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"TOWARDS A QUANTATIVE APPROACH TO AMERICAN INDIAN HISTORY"

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PREFACE

The papers from the McNickle Center's February, 1987 conference on quantitative methods in Indian history represent a departure from other titles in the Occasional Papers series. Typically, the Papers have addressed historical and contemporary subjects such as the American Revolution, Urban Indians or Indian Water Rights. This volume focuses on method.

We organized a conference on quantitative approaches to American Indian history in part because it seemed amazing that such a meeting had not already taken place. After all, during the past generation, historians and anthropologists have employed all manner of statistical and numeric measures to examine peoples who have generated few written records. Scholars have used these measures to illuminate the experience of groups as disparate as European peasants, Afro-American slaves, and industrial workers. Why not apply the same techniques to the Native American past?

But the goal of the conference was not simply to be innovative. Scholars have long been aware that American Indian history is hampered by the fact that the bulk of the written records they use have been generated by non-natives. What is typically available is a white man's account of what happened rather than an Indian version. Once these accounts have been corrected for ethnocentric bias and linguistic ignorance there is often very little left. While quantitative records were also largely compiled by non-Indians, they record activities and realms of behavior that are often poorly

described in narrative accounts. It is possible, we reasoned, that a quantitative approach will open new aspects of the Indian experience to scrutiny and study.

Finally, the conference provided an opportunity to break down the parochialism which is an inevitable part of any field. By consciously employing the methods used by other scholars studying similar historical problems, the conference participants hoped to gain a fresh perspective on their subject.

The following papers and commentaries provide evidence that the conference objectives were largely met. John Moore opened the gathering with a defense of quantitative methods and attack on scholars who ignore what he called "scientific" thinking. He was followed by four speakers who each presented an example of how quantitative methods might be applied to Indian history. Each speaker, including Moore, received comments on his paper from a quantitative historian who has little or no experience with Native American subjects. Each of these papers--and three of the commentaries--appear in the pages that follow. Finally, this volume contains three statements by members of the panel who spoke on the final day of the conference. Each speaker has written extensively on Indian history, but none of them have been particularly interested in quantification. The question for them was this: "How will the methods demonstrated at this conference affect "future research in the field?" Their statements explore both the promise and the shortcomings of the quantitative approach.

As always, this volume of conference proceedings benefited from the efforts of many people. All of the speakers responded nimbly to deadlines and other procedural requirements. Compilation of the papers themselves benefited from the assistance of Rose Summers and Colin Calloway of the Center Staff, and the word processing services of Jeff Auld, Rosemarie White, and Marilyn Deberry. The conference and this Occasional Paper were made possible by a grant from the Research Division of the National Endowment for the Humanities.

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OCCAM, NUMEROLOGY, AND THE PROBLEM OF SCIENTIFIC HISTORY

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Let me first express my appreciation to Fred Hoxie and the Conference planners for the privilege of giving the keynote address. I have taken this responsibility very seriously, and in the following remarks I try to raise the issues which underlie all our efforts at making sense of the ethnohistoric and early reservation periods for native Americans on our continent. I try to raise these issues in a manner which is provocative and will promote some debate and discussion. Also, I call attention to some published works in which these issues are debated and discussed at greater length.

I realize, of course, that in speaking to this Conference about the need for quantification in history and social science I am no doubt speaking to the converted. So please regard these remarks not so much as preaching to the heathen but rather as bringing comfort to the afflicted. For we are all daily afflicted with the writings of scholars who not only fail to incorporate quantitative methods where they ought to, but who sometimes often explicitly eschew what they call "number-mongering" and who sometimes even, forgive them, deny that history is or ought to be scientific. So my purpose here is largely to structure and put into words some of the things that we are all thinking, and to provide some published examples and personal anecdotes which illustrate both good and bad quantification in the cause of scientific history.

Perhaps surprisingly, I wish to begin with a discussion of some ancient philosophical issues, and with an endorsement of the importance of philosophical discourse for the disciplines of history and anthropology. For some reason, the study of philosophy seems presently unfashionable among colleagues and students. But we should remember that the leading scholars in our disciplines have often been people familiar with philosophical issues, and I have also found that students who profess boredom on being forced to consider such antique subjects as reductionism, tautologies, and false syllogisms, are soon observed to submit theses and term papers which are full of reductionism, tautologies, and false syllogisms.¹ So I want to begin with some crucial although ancient philosophical problems, and the initial and fundamental issue I want to address here is that of idealism or mentalism versus materialism.

In his address as retiring President of the American Anthropological Association in 1977, Richard Adams discussed how the "dichotomy between mentalism and materialism tears at the intellectual binding of our discipline."² But there was a

strange reaction to Adams' warning--despite the essential correctness of his analysis, then and now, his talk was simply ignored, although it was soon dutifully published in the American Anthropologist. It may be that he raised an issue so sensitive--like cancer or nuclear war--that his colleagues didn't want to talk about it. Or it may be that many anthropologists don't see their differences as based on some objective philosophical issue which can be resolved by honest discussion. In any event, the situation seems to have got worse in the last ten years, with the major protagonists either not talking to each other altogether, or, what is worse, becoming more histrionic in their mutual criticisms.³

To scientists in other disciplines, however, it must seem strange that anthropologists should even feel a need to debate a philosophical commitment which they take so much for granted--the materialist assumption that the real world exists, and that we are capable of measuring it to reach scientific conclusions. While the opposite philosophical extreme, philosophical idealism, ranges in scope from Kant to Mach to Berkeley, the extreme position must be that embodied in the ideas of William Blake, who held that "all created things whatsoever are only imagined, just as all dreams are."⁴ Perhaps there are inorganic chemists or nuclear physicists who are theists or idealists or even Blakeans in their private lives, but their disciplines force them to leave such opinions at home when they go to the laboratory. But strangely, in anthropology and history, one can still survive and prosper as a scholar while maintaining mystical and idealist philosophical commitments from Sartre to Schopenhauer, not to mention, in the case of Gregory Bateson, Lao-Tze and William Blake himself.⁵

It is often the case that intellectual currents proceed in parallel ways in different disciplines, or that there is a rippling effect from discipline to discipline, as with Freudianism and Structuralism. Recently, in the last decade or so, there has been an increased interest among literary and esthetic critics in romantic or imaginative writers such as Blake and Yeats, and I think there are some effects of this interest on anthropology, most notably among the members of the newly-organized Society for Humanistic Anthropology, influenced largely by the ideas of Marshall Sahlins and Clifford Geertz.⁶ Both these anthropologists, for example, speak of culture in the same manner as literary figures have discussed the Great Memory or Nature as some kind of invisible cosmogone.⁷

When we realize that some of the literary idealists, such as Blake and Yeats, were also occultists and mystics who went to seances and talked to spirits, we should be immediately alarmed that Geertz and Sahlins, with their hermeneutics and poetics, might be perpetrating the same kind of philosophical commitments in anthropology. I hope that I am not speaking just for myself when I say that I see this Conference, in part, as a response to this growing and alarming trend toward mentalism and mysticism in anthropology.

In these remarks, of course, I do not mean to paint all the various idealists with the same brush, although I wish to emphasize that they are qualitatively all the same. All of them allege that we must consider the Great Unknowable as we seek to describe the knowable. But there are important differences between Mach's skepticism about the results of laboratory experiments, and Blake's assertion that we all live in a dream world.⁸ In his useful book, The Case for Idealism, John Foster divides the idealists into three categories--mentalists, anti-realists and reductive phenomenologists.⁹ All of these, however, no matter how mild or radical their idealism, share the conviction that attending to data derived from our senses is not the best way to try and understand the workings of the world.

So I do not wish to fault idealists such as Geertz and Sahlins for what they are, for what they are is provocative, interesting, literary and stylish. Rather, I wish to fault them for what they are not. They are not scientists; they are not dedicated to reaching shared conclusions about the world by the logical analysis of data. In their work, there is no hypothesis, no explicit methodology and no data base. Consequently, there are no conclusions and no chance of challenge or replication.¹⁰ Instead, we usually find on the first pages of a humanistic work some suggestions of a hypothesis, which is then confounded on subsequent pages by all kinds of other intersecting hypotheses. Then there are some fragments of data, selected by who knows what methodology, followed by a carefully orchestrated conclusion, which usually only endorses the original hypotheses, except now from a loftier perspective. I submit that this is not science, it is literature.

In exhibiting their share of idealist and romantic tendencies, it seems to me that historians are both better off and worse off than anthropologists. The commitment to idealism seems more pervasive in history, but less extreme. For example, I do not read any history which is as strange as Levi-Strauss' statement that it makes no difference, ultimately, whether we are studying primitive social organization or the structure of our own heads, nor do I see anything in history as truly peculiar as psycho-archeology, by which the investigator squats on some archeological site and intuitively feels the life and times of past peoples.¹¹ But on the other hand, neither do I read any history which is as committed to empirical data, as rigorous or as scientifically fertile as paleontology or historical linguistics in anthropology.¹²

Daniel Gross, until recently staff anthropologist for the National Science Foundation, noted last fall how anthropologists, far from constituting a community of colleagues with shared goals, tend to form armed and hostile camps of combatants, shooting down each other's grant proposals, to the detriment of everyone.¹³ By contrast (from my admittedly less-informed perspective as an anthropologist), historians seem to me to be closer together in their ontology, their perception of the ultimate goals of history, and more homogeneous in their

methods.¹⁴ Which is not to say that there is no history which is not offensive to a materialist commitment. Foucault's analysis of the Renaissance, in which reified but disembodied Ideas go flitting about Europe, lighting first here and then there, is an insult to any materialist methodology, as is Namier's endorsement of the subjective element in historical writing. He says "History is therefore necessarily subjective and individual, conditioned by the interest and vision of the historian. His interest if intense and sincere is contagious, and the test of his originality is whether it is convincing."¹⁵ This seems to me to be nothing less than a license to lie, with penalties applied only to those who are not believed.

I cannot resist noting here another apparent characteristic of historians, once again speaking as an outsider. There seems to be a dichotomy in history between what scholars write for each other, and what they write for the general public. For themselves, historians write closely-argued empirical studies in which quantification seems to have a leading and ever-growing role. But for the public, historians seem to write tracts which are of a different order entirely, in which hero-worship and patriotism are still major if not always explicit themes. Much of popular history still seems to me to smack of what the poet Byron complained about long ago when he said that the world's great kings and robber chiefs lacked nothing at all, not even "History's purchased page to call them great."¹⁶

And in writing histories full of heroes and high motives, popular historians still fall back on the idealist inventory of concepts, where the motor of history is not anything as mundane as technology, political economy, or human fertility, but personal character propelled by motives like ambition, patriotism, greed or altruism, concepts which are not in the materialist inventory. Heroic history and history for hire are often criticized as if they were two different things, but I have never seen one which was not the other. Nowadays, however, the mode of payment can be very indirect. At the University of Oklahoma, for example, if you want to write a complimentary history of the oil or cattle business, your reward will surely come, although no one will hand you a bag of gold as in the time of Byron's robber chiefs.¹⁷

Anthropology, as I have said, has long provided haven for explicit idealists or mentalists, such as Frazer, Levy-Bruhl, Schmidt, Benedict, or the new romantic movement. But our discipline also comprises some curious and seductive philosophical hybrids, of which the most interesting must be Marvin Harris' "cultural materialism." With little effective criticism from others, Harris has succeeded in marrying the notion of materialism to the obviously idealist concept of culture. It is a seductive combination because he would seem to have invented a new variety of materialism, and left inviolate the canons of empirical science. But logically, the concept of cultural materialism is dramatically contradictory, as odd a

juxtaposition as, say, metaphysical chemistry or occult hydrology.¹⁸

The fact that culture is an idealist concept can be easily told if you try to find some of it. On those rare occasions when culturological theorists seek to "explain" culture, there usually emanates some kind of list of things which constitute culture. The lists customarily comprise things which are, qualitatively, wildly divergent from one another, such as curved knives, monotheism and matriliney.¹⁹ Somehow we are supposed to be able to tote up this list of things as the "culture" of some society. But no one to my knowledge has yet produced or toted up any quantity of culture in the real material world so that we can all see what it looks like. It always seems to remain in the ethereal world of the idealist theoretician, waiting for the next abstract discussion or the compilation of the next list.

It seems to me that the acid test for the difference between idealist and materialist concepts must be the question of whether you can lay hands on your subject matter. A human being is part of the material world, as are his tools, domicile and environment. A clan or a nation is an organized group of material human beings, whose behavioral structure we try to model. But you cannot grab culture, or national character, or Dionysian tendencies, and see how much of it you have got.

Having criticized some of the idealists in social science, I must also say that I don't think there is much to be gained by endlessly poking fun at the likes of Sahlins, Harris or Foucault, although we are all prone to do so. We materialists have our own agenda, and we don't need their permission to proceed with it, and we are not likely to convince them to help. Although there is great value in criticizing the mentalists and clarifying the philosophical differences between us and them, after that I think we should let them lie. Many scholars have wasted their energy in lengthy and sometimes hysterical attacks on theoretical opponents, when their time would have been better spent in making a positive contribution to their own concrete area of interest.²⁰

As materialists, then, we exclude from our consideration a huge number of questions which we know we cannot answer, except in subjective terms. But even though we limit ourselves to the relatively modest task of finding regularities in the observable, sensible, real world, we have still outlined a scope of work which will take more than our lifetimes to accomplish. Although it may not be as satisfying, emotionally, to delineate a modest history of a single tribe, as compared to making generalizations about "human nature," "the psyche" or "the nature of culture," still it is perhaps better to know a little bit for sure, than to make colorful literary statements about the ill-conceived and the unprovable. And we have the example of the physical sciences to show that an understanding of the fundamental and the profound most often proceeds from a consideration of the small and provable--genetics from the study of pea flowers, evolution from a tour of the Galapagos, and nuclear physics from the study of strange substances which glow in the dark.

However, a commitment to materialism does not, in any sense, solve all of our analytic problems, and if we doubted that we should look at some of the contortions in which we find some of our colleagues who profess materialism. Let me use as two examples the structuralist Marxists in anthropology and the kind of Marxist historian that Bernstein called a "Calvinist without God."²¹ The former commit idealism in the name of materialism, having never understood the difference, and the latter have reduced human history and evolution to a preconceived series of named stages. They perceive their scholarly task as merely determining which society is in what stage during which years. Instead of using Marxism as a flexible method for understanding human history, they see it as a formula within which to place their alleged facts. Is Morocco feudal? Were the Danubian peoples matriarchal? Was China ever capitalist; is it now? Were the Montagnais in the eighteenth century petty capitalists or primitive communists? It seems to me that much of this kind of scholarship is merely playing the kind of word games which Wittgenstein exposed in his Tractatus.²² These materialist scholars, having escaped the pitfalls of idealism, have fallen instead into the traps of reductionism and nominalism, philosophical problems which I now intend to discuss. These are, I think, next to the idealist-materialist dichotomy, the most important issues we must face as historical scholars who aspire to be scientific.

Reductionism

In anthropology, the traditional whipping boy for those who wish to criticize reductionism is Bronislaw Malinowski. Under the guise of what he called "a scientific theory of culture," Malinowski undertook to sort and label every aspect of human culture and "explain" them as expressions of a few basic biological drives--the needs for sex, hunger, physical movement, companionship, etc.²³ Of course the problem with Malinowski's reductionism, as with all reductionist schemes, is that it does not discover anything new. The fact that people cooperate to make gardens because they anticipate hunger, or that they marry because they desire sex and children, is something that we all knew in the first place. The process of reductionism, by which we tack labels on everything we already know, and explain each in terms of the other, does not tax our minds to the point that we create new theories or discover anything. And the key word must be "discovery."

Another reductionist scheme, now very much in vogue, is Wallerstein's World Systems Theory.²⁴ In reading Wallerstein's work, and despite his eminence and large following, I really do not see that he has discovered anything. He and his associates have merely invented an idiom and applied it very widely, both geographically and chronologically, comprising even archeological data. But anyone who has read twentieth century studies of imperialism, beginning with Hobson, will not find much that is new.²⁵ Only a new idiom.

If we reject reductionism, as we must, as a method which does not discover anything, it therefore follows that we should be looking for methods which do tax us and force us into the discovery process. But to discover, we must still label, name and categorize things, just as reductionists have done. But the difference is whether we apply pre-conceived categories, or whether we are discovering categories which already exist in the natural, material world. It is the difference between "discovering" that the ancient Chinese were feudal, and discovering the site of Qufu. But let me give one very modest example of the discovery process from our recent research on the Cheyennes in the early reservation period.

In our research project, a graduate student named Dori Penny was given responsibility for what we called the "beef band lists." These date from the years 1876-1888 and contain the name of each band leader receiving beef, along with his age and the number of people in the band. To approach this data set, Dori Penny conceived the idea of plotting the age of each leader against the size of his band, hoping to discover a band cycle in which the older, longer-established bands would be larger, and the new, younger bands would be smaller. We would expect to find a process of reorganization at the point when the leader dies and the band has to reorganize around the children of the dead leader as two or more smaller groups.

And indeed, when the data set was entered and sorted, Dori found the expected correlation between age of leader and size of band, but the surprise was that the points occurred not in one cluster but in two.²⁶ And so we were led to **discover** that there were two "types" of bands in Cheyenne society, something we did not expect to find. By doing some additional work linking collateral documents, we found that the cluster of points representing smaller bands constituted a cycle of development for monogamous extended families, while the cluster of larger values represented the cycle of Chief-led polygynous extended families.

So here we have "discovered" something, unanticipated, and it is easy to congratulate ourselves for being real scientists. But this would be very premature, because both in creating our analytic categories and in verbally characterizing the results, we have perhaps unknowingly unleashed all kinds of philosophical problems which we may perceive only dimly in the excitement of handling data and the flush of discovery. These problems fall under the general rubric of "nominalism," which is the issue I want to consider next.

Nominalism and the North American Data Base

In the Western philosophical tradition, we have a whole series of philosophers to thank for calling to our attention that things are only what you call them.²⁷ Nominalists, of course, are necessarily opposed to idealists such as Plato, for example, who claimed that words represent essences or "forms" which we poor humans can only adumbrate in the darkness of our cave.²⁸ Nominalists, by contrast, are humanists, who see words as human creations, tools for the accomplishment of human tasks. Long

before cognitive anthropology discovered that no morphemes for colors, body parts or biological species could make any claim to being universal, the Greek philosopher Diogenes reported that he had visited Plato in his home and had seen his "cups and table, but not his cupness and tableness."²⁹

Nominalism has at least two propositions which should interest us as scientists. First of all, nominalism tells us that words are arbitrary labels which are pure symbols, and which we can change at our whim. But on the other hand there is a certain nihilism or cynicism in nominalism when it is stated that it **doesn't matter** what you call things. From the standpoint of building scientific theories, I think it matters very much.

When Dori Penny discovered two clusters in her data base, for example, certainly it is true that we could have called them anything we liked. We could have called them big families and small families; we could have called them blue families and green families; in fact, we could even have called them duck soup and chicken soup, and still have satisfied those philosophers who call themselves radical nominalists. But we chose to call them monogamous extended families and polygynous extended families, because we wanted to **bind** our discovery from this data base to other data bases which other scientists might be concerned with. That is, we tried to select a descriptive language which would communicate most effectively with other people involved in the same kind of scientific work. The difficulty with the reductionist schools of thought is that they often develop a unique language of analysis which not only obscures the fact that they haven't discovered anything, but also prevents any communication between them and people involved in similar investigations.

Two pitfalls of nominalism are at the end of our scientific project, when we characterize our results, and perhaps more importantly, at the beginning, when we categorize data for analysis. And here we should take a close look at the kind of data we customarily use in examining the early documentary period of American Indian history.

It would be extreme, I think, although it is in some ways tempting, to allege that administrative censuses, letters, and lists of various kinds from the early reservation period constitute a natural data base. That is, it is tempting to say that these documents and maps are like stratified soils or the anatomy of a bird, unsullied by the interference of the investigators, with their questionnaires and field notes. And in fact, the beauty of such documents for ethnologists is that they are terribly naive with regards to the questions we wish to ask of them. In this, they contrast agreeably with the observations of North American ethnologists in the early reservation period, who **always** had some bone to pick about the most salient characteristics of Indian cultures.

By contrast, the administrators who rostered Indian people and struggled with them in the early period of contact, were largely ignorant of the problems which now interest us as

ethnohistorical scholars. Which is not to say that these administrators were not ethnocentric and bigoted men, for they largely were, but only to say that they frequently were not intellectually distorted in any manner which pertains to our scientific interests. For example in the 1917 competency interviews, one frequent entry is the notation "old and ignorant."³⁰ What I am saying here is that for most scientific purposes it doesn't matter that the enumerator was a racist and regarded certain Indian elders as "old and ignorant," as long as he got the composition of the family right, and correctly counted the total number of horses in the pasture.

In their important book, Systematic Empiricism, Willer and Willer show how social scientists can insure their results in advance, simply by the way they ask their questions and create their categories.³¹ It is, I think, the single greatest failing of field ethnography that we trust field-workers to collect the data which will support or falsify their own theories. Two problems usually result from orthodox fieldwork: first of all, the fieldworker usually only reports what happened to interest them, and secondly, it is often impossible to refute a person's theories using their own data. Witness the recent dispute over Margaret Mead's allegation of non-violence among the Samoans.³²

So the beauty of our data on North American Indians is that it is not only naive, with regards to the theories we wish to test, but it is also very comprehensive. There are literally rooms full of administrative documents of the early reservation period. However, to use this material properly, we must recategorize it--for example the entry "old and ignorant" from the competency interviews. If we categorize the column simply as "old and ignorant," in those words, we shall discover nothing. It may be, however, that the people so noted are persons who represent some social or political category which does interest us. Perhaps they are people who resisted the interview for some political reason, who were older than a particular threshold age, or who were involved with traditional religion or healing.

In a published article, I have tried to show how such apparently irrelevant entries on official reports can tell us whether certain Cheyenne leaders were "agnatic" or "uterine" in their social behavior, and were militants or compradores in their political conduct.³³ So the problem, as I see it, is that of sorting and translating naive administrative categories into purposeful scientific ones, and we should not be discouraged at the often baroque appearance of the naive categories, before we apply to them the purposeful hypotheses which can cause them to exhibit scientific results.

Parsimony and Scope

So far in these remarks, I have not mentioned William of Ockham by name, and in fact I have largely defined what scientific history is by applying the process of exclusion, by explaining what it isn't. But let me now proceed in a more positive manner, and discuss the important, indeed crucial contributions of Ockham to the progress of science. Ockham was,

first of all, one of the framers of the nominalist principles outlined above, and he also helped identify and criticize reductionism. But that is not what he is mostly remembered for.

It is often the case that the classic contributors to Western thought are best known for things they never said. I first discovered this on reading Durkheim, alleged to be the originator and paradigm of staid, synchronic, anti-historical functionalism. But I found that his book The Division of Labor in Society is all about the dynamics of social evolution. Soon after that, I discovered that Karl Marx was not an economic determinist, and since that time I have seldom trusted any colleague who attempted to characterize for me some classic book or article which is supposed to influence my thinking.

So I was not surprised, on reading Ockham, to find that he is usually only credited for half of what he contributed to the growth of science.³⁴ We all remember his principle of parsimony, or more correctly, "principle of economy," the statement "pluralitas non est ponenda sine necessitate." Very simply, this principle states that when in science we are confronted with two theories which are equally successful in describing a set of phenomena in the natural world, we should embrace the simpler theory and reject the other. In fact, Ockham got into hot water with the Church by suggesting that this was all there was to the idea of "truth." Something was true if it economically explained some set of phenomena, and false if it didn't. Of course, the Pope and the Church had somewhat different ideas on what constituted truth and where it came from, and Ockham was constantly being investigated and asked to explain his views in the fourteenth century. To credit Ockham properly, we should put him into this historical context. In the period from Aquinas to Luther, the Church was fighting for its intellectual life against currents created by rising mercantilism and capitalism, both of which required active scientific inquiry for their continued success. In this period, the Church burned Bruno at the stake, thereby upholding the "truth" of geocentric astronomy, and that the Popes had opinions on every subject from anthropology to zoology. For example the Pope proclaimed in the thirteenth century that human thought proceeded from the soul and had no basis whatever in the physical structure of the brain, which of course settled this question for all time.³⁵

Regarding the intellectual effect of Ockham's writings in this proto-Renaissance period, we must note that it is his principle of economy which allows us to choose, for example, between the astronomy of Copernicus and that of Aristotle. Instead of the enormously complicated algebraic and geometric calculations which the Aristotelians thought were necessary to explain the positions of the planets in a geocentric system, Copernicus gives us a simple, heliocentric model, with some diagrams and equations. And that is what Ockham says truth is, and that is all that it is. Whatever human sentiments and loyalties we append to our theories, they in fact, according to Ockham, are "true" only because they represent a simpler and more

logical manner of looking at the natural world. But that is only half of what Ockham said, and the other half is equally important.

Taken by itself, Ockham's Razor can get us into a lot of trouble. For it is clear that the best and the simplest theories are those which apply to the smallest and most trivial sets of phenomena. If we continually discard complex theories in favor of simpler ones, we shall be more and more certain about less and less. Anticipating that, Ockham argued, with equal strength, that those theories are more powerful which explain more phenomena.³⁶ That is, he saw theory and empirical data in a dialectical interplay, between the attempt to embrace the largest class of phenomena, and the attempt to create the simplest theory. Ockham called these the interplay between scientia rationalis, the theory itself, and scientia realis, the application of that theory to natural phenomena.³⁷ We can also call these the interplay between parsimony and scope, between the simplicity of a theory and its breadth when applied to the real world of data.

Let me turn again to Dori Penny's discovery of two types of Cheyenne bands, to illustrate this kind of interplay, and to show how theories can be extended in scope. First of all, we can amend the theory and extend it within Cheyenne society by consulting other documents, such as the allotment maps of 1892. Here we find that the monogamous and polygynous bands were combined into two distinctive types of larger units, called manhao and notxestoz in Cheyenne. These two types of macro-bands polarized geographically in early reservation times between the northern and southern parts of the reservation. So we can state an enlarged theory which comprises geographical dispersion, kin type and native sociological categories as follows: In early reservation times the Cheyennes maintained their aboriginal social and political structure, polarizing themselves into patrilocal and matrilocal composite bands, each of which was divided further into polygynous extended families, manhastoz, and monogamous families, vestoz.

So here we have a theory which describes a great part of the social structure of Southern Cheyennes in these times. But how can we extend the theory even further, beyond the tribal boundaries of the Southern Cheyennes, to make it more powerful, according to Ockham's ideas about "real science." That is, how can we extend the **scope** of the theory to make it pan-tribal, so that it has application beyond the Cheyennes.

One way to extend scope might be to hypothesize that all Plains Indians organized themselves into antagonistic kin-based composites of monogamous and polygynous families and that they were factionalized by a polarization into matrilocal and patrilocal groups in the early reservation period. If this were true, it would be a significant addition to our understanding of the ethnohistorical period. It would be a gateway to other kinds of theories regarding pre-reservation ecology or the generation of compradore or "progressive" factions on the various

reservations. But the enlarged theory must be tested, and one way to test it is by reference to the respective allotment maps of the various Plains tribes.

Only a little research, however, will tell us that in attempting to enlarge the scope of this theory, we have made it false. Tom Kavanagh's research with the Comanche allotment schedules, for example, seems to show the existence of bilaterally-recruited extended families, drawn into larger composites by some uniquely-Comanche pattern of political alliances among senior men.³⁸ Dan Swan's analysis of Osage allotments is also counter-exemplary, since it seems to show residence groups based on membership in the discrete peyote tipis of the Native American Church, with ambilineal and ambilocal residence rules defining memberships of the discrete groups.³⁹

Although from such results we are disappointed in trying to extend the Cheyenne theory to other tribal nations, the solution to our disappointment is not to shuck the whole idea of theory-building. If we do that, we are beginning to share the theoretical position that every culture has its own rules and can only be understood in its own terms. This assumption, a common one among ethnologists, is extremely anti-scientific. Once again, as with reductionism, the issue is whether we intend to build bridges between societies and increase the scope of theories, or whether we wish to accede to the idea of cultural uniqueness and create tight little culture-specific analytic compartments. So when confronted with difficulties in extending scope, our solution must be the one that Ockham suggested--to modify the form and extent of the theory, perhaps by nominal means, changing the words we use to describe results, so that we can achieve the most parsimony and the most scope possible, given the data.

To return to our example, the following three theories, reconceived and reworded, seem to be "true," even when we extend the scope of the Cheyenne experience to include the Comanches and Osages: 1) Aboriginal extended families continued to exist into the early reservation period, 2) These families were organized into composite bands on the basis of kinship, 3) the allotment schedules and maps accurately reflect the membership of real social units at the time of allotment, and 4) early reservation factions were cleaved along pre-existing lines of structural organization. Less certain but still provocative is the hypothesis that the aboriginal macro-band structures of all Native American groups continued into early reservation times.

Ultimate Units, Null Hypotheses and Scientific Communication

In building parsimonious theories and testing them against an empirical data base, it is important to note a fundamental qualitative difference between our data and the kind of data analyzed in the physical sciences. In chemistry for example, the ultimate unit of analysis is the atom, and as far as chemists can discover, all elemental molecules of the same atomic number and atomic weight behave exactly the same. In nuclear physics, the ultimate units are sub-atomic particles like mesons and bosons

and quarks, although physicists are still trying to discover how many there are and how they are related. But the ultimate units of analysis in the social sciences are individual human beings, who we know from the outset are behaviorally different from one another--in fact there are no two alike--and none who are exactly predictable.

The importance of this observation, for quantitative analysis, is suggested in Levi-Strauss' postulated difference between what he calls mechanical and statistical models.⁴⁰ Mechanical models are for theory and statistical models for analysis of the real world, the same distinction noted by Ockham in his contrast between "rational science" and "real science." In mathematical theory, the difference is between axiomatic techniques, which proceed from supposedly iron-clad premises, proofs and assumptions, and the techniques of probability, which operate in an environment of bell-shaped curves and Monte Carlo assumptions. While it may be true that we can generate hypotheses about human beings by axiomatic techniques, the proofs must be from statistical techniques, because our ultimate units of analysis are non-identical. In Levi-Strauss' own work, however, and despite his theoretical insight, he consistently takes his mechanical hypotheses as self-evident, without supplying any empirical evidence analyzed by statistical methods. The converse error, which I shall discuss in a moment, is to take statistical trends from an empirical analysis, and allege that it constitutes a theory or hypothesis about the real world, when in fact it is only an axiom about a methodology.

Although I don't wish to become too abstract in this short discussion, the issue of randomness is important enough to mention, I think, especially in light of the nature of our administrative data from North America. Theoretically, it seems more and more apparent that randomness in nature is hard to find, since even chaos seems to be structured, according to the published work of Edward Lorenz and Robert Shaw.⁴¹ It follows, then, that any representative sample of nature must also be structured, so that any assumption of randomness in a natural data set must be regarded as very questionable. This must be especially true of our administrative data sets, since we most often cannot account for the methods used to create them, or the reasons why the sets are incomplete. Consequently, we can use statistics based on the idea of randomness only at the peril of later falsification. It is more important, I think, to test our data as to whether it is structured with respect to our hypotheses, and whether it is sensitive to variables independent of our hypotheses, than to assert that either the data set or the sample is in any wise "random."

Let me also say a word about the value of null hypotheses and verification. Most often, I think there is as much to be gained from null hypotheses as positive ones. Although many of my Cheyenne hypotheses have not proved out against the Comanches and Osages, I have gained a great deal from Kavanagh and Swan. From Kavanagh I learned to look for the political consequences of

personal alliances, and from Swan I learned to look for residence groups based on ideological commitment, and in fact I have found three of them contiguously allotted among the Cheyennes in 1892, one Episcopal, one Mennonite, and one Sun Dance group.

Also I think, if we are to legitimate ourselves as scientists, we must cease to regard criticisms of our work as necessarily hostile. If someone else takes our data base and tries to get the same results as we did, it does not necessarily mean that they are trying to catch us in a lie. If we have played fair with the data, we should be glad of the verification, as strengthening the hypotheses under consideration. Sharing data and ideas helps differentiate us as scientists from certain other anthropologists and historians, who seem to guard their field, library and archival data as if it were Grendel's gold. So to help set a trend, let me here offer to share any of my data with other co-researchers, any time, as long as they agree to respect the privacy of my informants. I am not making this offer just because I am a nice fellow, for I have much to gain from sharing data. I especially enjoy sharing data with young scholars who have no data of their own, but do have a lot of good ideas. It is, I think, with this kind of trust, commitment, and interdependence that we can begin to build a real community of ethnohistorical scholars interested in North American Indians. This can replace the jealousy and hoarding of ethnographic resources which has so often plagued our disciplines, and hampered the advancement of ethnohistory as science. I think that, by and large, scientists share data, while humanists keep data under lock and key.

Numerology

I still have not discussed the last topic promised in my title, numerology, but let me do so now. Please notice first of all that in all the concise theories framed above, there are no numbers. And the truth is, if I may use that word "truth," that I am quite skeptical of the utility of numbers for doing history and social science. This might seem a strange thing to say at a conference on quantification, but let me explain how I came by my skepticism.

I admit that for some years now I have been lavish in my use of mathematics, graphics and computers in doing ethnohistory, as will be shown in my forthcoming book, which has nearly a hundred tables and figures of various kinds. But I should explain that I have used these methods largely because they were familiar to me, because of my undergraduate training in chemical engineering. The curriculum required that I take math up through differential equations and statistics. As a practicing engineer for a while, I used statistics and computers in my work. Like most engineers, then and now however, I considered mathematics to be merely a tool of my trade, an imperfect and often contentious tool, but still useful.

Imagine my surprise upon entering anthropology in 1964 and witnessing the nearly worshipful attitudes which many social scientists maintained toward the field of mathematics. In

anthropology, so I discovered, there was almost no questioning of the axiomatic bases of math and statistics, or any recognition of the arbitrary and teetering nature of the assumptions on which theoretical mathematics was based.⁴² While in the math department they know full well that parallel lines often meet, and that the jury is still out on such basic issues as infinity and negative numbers, in anthropology these questions have apparently already been settled. I once asked a graduate student to plot for me a "linear curve" and was told that there was no such thing. On another occasion I used the equation describing a normal distribution curve to estimate some population statistics, and was told by a colleague that the equation must be wrong because he couldn't find it in his book. With co-workers I frequently discuss the "variance" in natural data sets, and I once received a long dogmatic memo from an archeologist explaining the sanctity of the denominator "n-1."

There also seems to be a rule for the use of math in the social sciences which is the opposite of the rule in the physical sciences. While physicists, for example, try to use the simplest math which is applicable, the rule in anthropology seems to be, Don't use arithmetic when you can use algebra, and don't use algebra when you can use calculus. There are many illustrations I could use here; let me take first a very recent one, from the ethnobiologist Brent Berlin.⁴³

Berlin's theory of the development of ethnobiology, if you are not familiar with it, is that human societies have mostly created their plant and animal taxonomies in situ, first developing a few simple terms for large classes of living things, and then elaborating that scheme to more specific and more numerous categories of "families" and "genera." What is wrong with the theory, as I have recently stated at greater length, is that for most societies it flies in the face of the archeological and ethnohistorical record.⁴⁴ We know, for example, that the Aztecs are from the North American steppe and that the Navahos are from Alaska, not to mention the fact that all American societies over the last 20,000 years or so, are from Asia. So unless Berlin is assuming that all these peoples arrived mute, and learned to speak only after they got here, his theory seems to be of very little historical value. But nevertheless, in his most recent offering, Berlin obscures the essential weakness of his theory behind a cascade of multivariate analyses, ending in a flourish of logarithms.

Another peculiar example of mathematics at work is Mosimann and Martin's "Simulated Overkill by Paleoindians."⁴⁵ Here the authors use probability statistics to model a situation in which groups of hunters fan out from Edmonton, apparently the center of Paleoindian culture, at an assigned migration speed in discrete compass directions. In ordinary English, what they are postulating is absurd. In terms of real behavior, it calls to mind the image of a Paleoindian band leader, compass in hand, marching exactly 10 kilometers southeast, eating everything in sight and then waiting for further instructions. But the

absurdity of the scheme is obscured, as with Berlin, by a great cloud of statistics and tables. In sum, it is a model masquerading as a theory, a model which has no proven relation to the material world.

One more example, and in this one there is no hypothesis to test, not even a foolish one. In this case, the statistics are mongered for their own sake, and a methodological axiom takes the place of theory. I refer to the article "Multidimensional Analysis of Pitjandjara Kin Terms Usage: HOMALS Representation" by Tjon Sie Fat.⁴⁸ In this article, the author tells us that he is going to provide "intuitively appealing representations of the underlying structure of the data by mapping both the individuals and the kin terms onto the same geometrical space." Mind you, the author does not set out to solve any real problem in the real world, but merely wishes to expound a methodology. He confirms this in what he calls his "conclusion:" "My argument is essentially a plea for introduction of more formal techniques...The use of HOMALS to elucidate the structural properties of the Pitjandjara tabular data should be seen as a demonstration of the possibilities..."

So here we have a statistical technique masquerading as an ethnological theory. We might as well have a theory of means, modes or standard deviations. How about this one, which I have made up? The Crow Indians, in the period 1840-1888, consistently maintained HOMALS dimension scores of between 2.45 and -5.67. Does this kind of statement tell us anything about Crow history; does it raise our curiosity about the HOMALS scores of neighboring peoples; does it open the door to new discoveries? I think not.

It is examples like these, I think, which have given quantification a bad name among some of our colleagues. Some of the critics of quantification are merely unreconstructed Luddites, I admit, but many of them are quite sensible people who recognize mathematical malarkey when they see it. I suggest that our task as **scientific** quantifiers is to use math as little as possible, and then only in a modest and sober manner.

Some of our high-tech colleagues, I fear, remind me of the cargo cultists of the South Pacific. These religionists, as you know, build huge grass and bamboo airports and warehouses in the hope that their ancestors will land and bring them cargo for the benefit of their people, just as the airplanes of the Europeans land and bring cargo to the Europeans. In the same way, the mathematical super-sophisticate builds an elaborate artifice of formulae, tables and graphs, in the vain hope that Truth will somehow find his futuristic construction, be impressed and land in the middle of it. But I think that what we need for scientific history is less math and more clear thinking, buttressed by those mathematical techniques which are not the most complicated possible, but the most appropriate.

Let me also put in a plug for verbal as opposed to symbolic or numerical theories, which is to say, "equations." I believe there is a common misconception that verbal theories are somehow

less exact than numerical ones. But I don't understand how anyone could still believe that since the publication of Principia Mathematica in 1910.⁴⁷ It is more accurate, I think, to say that verbal theories have more potential to be vague and ambiguous than numerical theories. But still, the following verbal statement by Fred Eggan, for example, is very precise: "...the Santee once had cross-cousin marriage but gave it up for various reasons, including increase in population density and village size..."⁴⁸ Many other such examples can be winnowed from the writings of the more scientific ethnologists and social anthropologists. But contrast those statements with the following incredibly vague verbal theory, from Sahlins, "...an event is a unique actualization of a general phenomenon, a contingent realization of the cultural pattern...On the other hand, then, as the contingent circumstances of action need not conform to the significance some group might assign them, people are known to creatively reconsider their conventional schemes."⁴⁹ What I have called "clear thinking" in this essay I define as the ability to discriminate between verbal theories such as Eggan's, which have clear reference to things and processes in the material world, and theories such as Sahlins' whose ultimate referents are, at best, idealist concepts, and at worst only the words themselves.

Confronted with the difficulties of reducing Sahlins' kind of idealist statement to modest and testable verbal hypotheses, it is no wonder that many social scientists have leaped over this analytic or "clear thinking" stage and found refuge in numbers and other symbols instead. One of the most optimistic statements about the power of numbers is probably the following from Joseph Fourier, expressed in 1878, "...mathematical analysis is as extensive as nature itself; it defines all perceptible relations, measures times, spaces, forces, temperatures; this difficult science is formed slowly, but it preserves every principle which it has once acquired; it grows and strengthens itself incessantly in the midst of the many variations and errors of the human mind. Its chief attribute is clearness; it has no [symbols for expressing] confused notions. It brings together phenomena the most diverse, and discovers the hidden analogies which unite them."⁵⁰ But while Fourier himself was, of course, a competent scientist, it does not follow that all who use numbers are necessarily clear and scientific in their thinking.

Faith in numbers has a long history in Western philosophy. It was probably Egyptian architects and builders who discovered certain regularities in nature: that the sides of a 30 degree right triangle were exactly in the proportions one to two, that another right triangle had the proportions 3 to 4 to 5.⁵¹ They, the Greeks and the Arabs discovered all kinds of other mathematical regularities, such as harmonics in music, the planetary epicycle, and the tetraktys. But in the minds of some Greeks, and some medieval writers, a mystical element was added to this faith in numbers, and it was argued, even by Newton and Kepler that what they were discovering was nothing less than

God's plan for the universe, made simple enough for human minds to understand.⁵² In addition, certain of the mathematicians asserted that numbers had the power to heal the sick, foretell the future, and kill one's enemies, and they were called "numerologists," to differentiate them from those mathematicians such as Newton and Fourier who were increasingly concerned with the material world of physics and astronomy.

It was in this period of euphoria about the power of mathematics that Fourier wrote the articles of allegiance cited above. But bad times were coming for mathematicians, as Georg Cantor and Kurt Godel began to show that most of math was tautology.⁵³ By 1920 it was probably only Gypsy fortune-tellers who were still confident that numerical theories always provided accurate and specific parameters and prognosticators for life in the real world.

But it is this kind of faith in the power of numbers, I fear, which persists in the social sciences long after it has disappeared in the discipline of mathematics. While we tend to look at mathematical handbooks as some kind of holy writ, mathematicians see mathematical formulae as theses to be challenged and falsified. But now there is a new allegiance in academe, an allegiance to math as embodied in computers. And certainly it must be said that research utilizing computers has already proven it can heal the sick, foretell the future and kill one's enemies. For we have computer-driven genetic analysis, multi-variable econometrics and complex Pentagon war games to accomplish all three of these tasks. No wonder, then, that many of our colleagues have enormous faith in the power of computers. Let me relate two anecdotes.

In 1983 a colleague at another university sent me a small data set that he wanted, as he said, "put into the computer." So I entered the set, as it was, and received a phone call a week later. "So what did you get," he asked. "Well," I answered, "I entered the data and put it into alphabetical and serial order." "And what did you get," he repeated. "I didn't get anything yet," I said. "Exactly what variables did you want me to look at, and what is your hypothesis." There was a long silence on the telephone. Finally he said, "Well, I'll get back to you." So I sent him his printouts and that was the last I heard from him about his data set.

Another anecdote involves a worker in my project, a good programmer who supervised main frame operations. Let's call her Mary Ann. Early in the project, we developed the idea of finding macro-bands by analyzing the order in which the extended families showed up to take their beef issues. Extended families who were members of the same macro-band, we hypothesized, would tend to arrive consecutively to take their beef issues.

To test this hypothesis, we decided to cluster the names of extended family heads appearing on quarterly beef issue lists from 1880-1882, a total of eight lists. After some initial problems getting the printer to handle our long list of names, the results finally began to appear early one afternoon. Almost

immediately, a very excited Mary Ann appeared in my office, splashed the printout on my desk and shouted, "Look at this correlation." And in fact, seven members of the data set had nearly perfect correlations together, indicating they always took rations together, or nearly so. As I continued to look at the printout, Mary Ann went dancing up and down the hallway like Thomas Edison, calling other members of the project to come look at the printout. It was my lot to dump cold water on the festivities.

"Mary Ann," I said, "the only reason these people cluster is because they only showed up once, in the fall of 1880, and they never came back." "What," she said, and began to look at the trailer and entry cards to see if I was correct. Meanwhile the other members of the project began to look at each other, and then grumble and drift back to their desks. Finally, Mary Ann said, "But their index of cohesion is very high, surely that means something." I pointed to the data and said, "It doesn't mean they showed up together, Mary Ann, it only means they all showed up just one time and never came back. They cluster only because we have but one event for each person." She inspected the data some more, and began to look very angry. "But still," she finally stated through clenched teeth, "a correlation of point nine three six is nothing to sneeze at."

Before I leave the subject of computers to conclude these remarks, let me try to say very succinctly what I think computers are good for. They are tremendous tools for digesting a huge body of data and re-ordering that data as necessary. In a manner of minutes, one can extract sub-sets, run all variables against all others as a discovery process, and measure the statistical significance of results. But the computer cannot create new categorizations of data for testing new hypotheses, it cannot judge the significance of a correlation in the world outside the computer, and most importantly, it cannot create new hypotheses or theories by corresponding with colleagues, talking with co-workers, or just sitting around cogitating. Our own brain, because it is full of irrelevant input errors, glitches, unsynchronized timers, such things as sex drives, unmastered foreign languages and fragments of rock music, is creatively superior to the computer because it is capable of thinking in an original, although often confused manner. But if we discipline our brain processes by reference to philosophical principles and mathematical axioms, we can still aspire to be scientific. By contrast, a computer is only a book, with the pages already written.

Summary and Conclusions

So let me summarize briefly the points I have tried to make in this short essay. Mainly I argue here that what we need to promote quantitative methods in the study of American Indian history is not so much more mathematics as more clear thinking. To achieve clear thinking, I have recommended that we commit ourselves to a materialist perspective, like all the other empirical sciences, and carefully excise from our theories any

suggestion of mentalism or idealism, for these inevitably lead to the creation of untestable hypotheses. Once we make the commitment to materialism, I argue, we must still be wary of the dangers of reductionism, by which we persuade ourselves that we have accomplished something when all we have done is to rearrange the obvious. I also caution against the false security we experience when we trust ourselves to mathematics and computers. And lastly and most importantly, I argue for special care in selecting the concepts and words we shall use to categorize our data, and to write theories which describe our results. Our goal, I assert, should be the same as that described by Ockham, to create elegant and parsimonious theories which have as great a breadth and scope in the real world as is possible. To accomplish this in the quickest and most congenial manner, I suggest that we form a community of like-minded scholars, and I see this Conference as one step toward attaining that goal.

And what are some of the problems we should be thinking about, as we apply correct philosophical and quantitative methods to our chosen area, North American ethnohistory and reservation history? Let me suggest a few. First of all, we want to identify, for all Native Americans, their aboriginal social and political condition. How many, and how many kinds of tribes and sub-groups were there, why were they separate from each other, and how were they related? Second, there is the more difficult problem of how many nations were there in North America at any one time, and what do we mean by a nation? How do we sort out the contradictions in the ethnohistorical record about the numbers and names of tribal groups? Third, in the reservation period, how were these aboriginal nations, tribes and sub-groups transformed into administrative units? As "progressive" and "traditionalist" factions were organized in this period, what was their structural basis? Were they broken from a nation on some traditional line of cleavage? Or did a "progressive" faction, as frequently alleged by their opponents, constitute "mixed bloods," orphans and renegades? And what are the modern consequences of these processes of factionalism? Does the process still go on, or are there modern, undiscovered new parameters of factional differentiation? It seems to me that all these questions can constitute hypotheses which are vulnerable to testing by quantitative methods from administrative records. I believe that the members of this Conference can provide the impetus for testing such hypotheses, as an organized collective body of like-minded scholars.⁵⁴

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54. My thanks to Morris Foster, Shelley Arlen, Fred Hoxie and Joe Whitecotton for criticizing this paper and helping me to improve it from a rough draft.

VARIETIES OF CLEAR THINKING:
A CRITIQUE OF POSITIVISM
IN NATIVE AMERICAN STUDIES

T.H.BREEN

If I had been asked to provide a title for my "comments" on Professor John Moore's paper, I might have taken liberties with William James's work and called it "varieties of clear thinking." This title not only focuses our attention on a central theme of Moore's essay--his appeal for "clear thinking"--but also reminds us that Moore sees the problems of doing "scientific history" in self-consciously philosophic terms. Indeed, he asks us to address ourselves to issues of profound importance, to reflect upon the core intellectual assumptions that inform modern history as well as anthropology.

Moore's summons comes as a propitious moment for both disciplines. As anyone who reads the professional journals has learned, social sciences in the late twentieth century seem obsessed with their own lack of direction. Some might suspect a massive failure of nerve. Younger scholars sometimes

assure each other that this intellectual uncertainty is actually liberating, that they have managed to free themselves from the confinement of traditional discourses which they proclaim are mere rationalizations for the exercise of power. But their confidence often rings with bravado, as if they suspect that they are really part of the problem rather than its solution. In this atmosphere various theories compete for attention, for acceptance, for legitization. Some modes of analysis are surely better than others, but in the late twentieth century, even the word "better" when applied to scholarship creates anger, guilt, and despair. There is a growing sense that something needs to be done. We look to other disciplines for answers that they never seem to yield; we flock to academic "gurus," demanding assurance that our words, in fact, mean what we intend, that texts can be decoded, that data collection is not a fetishistic waste of time.

Into this dispiriting intellectual environment charges Professor Moore. He is a Bobby Knight, it is halftime, and his team is behind. Moore has no sympathy with those who have lost faith. Indeed, he unashamedly talks of methodology in terms of "conversion." But as we quickly discover, before we can be saved, we must learn to identify heresy. And that is no easy task! Moore rails against "romantic anthropology", fuzzy Geertzian theories of culture, "idealist" histories in which hero-worship and patriotic dribble pass for analysis, "mentalist" fantasies, subjectivist nonsense, reductionist schemes, and vapid materialism! For starters throw out your Marshall Sahlins, Marvin Harris, and Michel Foucault; they will only corrupt your critical faculties. Be wary of the dreaded "untestable hypothesis."

The novitiate trembles before such a list of interpretive sin; surely, damnation is at hand.

But with the discovery of "science," light enters the dark cell. By confronting human behavior directly, by measuring it, counting it, appreciating its patterns, by running it through powerful computers, by subjecting it to sophisticated mathematical tests, by purging the inquiring mind of muddled-headed pre-conceptions, one can catch a glimpse of reality. If you collect enough "empirical evidence," you may experience a kind of positivist new birth, and one day it may be possible for you to enter an academic eden where right-thinking researchers share and compare their data. "It is...", Moore declares, "with this kind of trust, commitment, and interdependence that we can begin to build a real community of ethnohistorical scholars."

It is regrettable that Moore has chosen to formulate his argument in such polemical terms. I say regrettable not because I find his presentation offensive. After all, a strongly worded statement is often more provocative than the alternative. Rather, I stress regrettable because I fear that Moore's embattled stance, his busy ferreting out of the "mentalist" heresy, creates a falsely dichotomous perspective on the issues before us--as if the world of scholarship were neatly divided into two warring camps--and thus, I suggest, he misses an opportunity for synthesis, for creative reformulation, for an imaginative leap beyond what I sense has become a sterile discourse between two interpretive positions that are more rhetorically than substantively at odds.

One experiences, in fact, a sort of deja vu while reading this essay. Moore writes of a battle that has already been joined by men and women working in ancillary fields. The dream of establishing a "scientific" history can be traced back to Ranke and to other Continental positivists of the late nineteenth century. Since then, scholars have wrestled with the "world out there," or to put it in the vocabulary of philosopher Richard Rorty, they have struggled to show decisively that the human mind is a "mirror of nature."¹ This intellectual history need not detain us. It should be noted, however, that many leading historians and anthropologists are of the opinion that their disciplines have moved beyond this agenda. As Louis Sass explains in an article entitled "Anthropology's Native Problem: Revisionism in the Field," for example, "There is a growing dissatisfaction throughout these disciplines, a sense that time-honored methods and assumptions, based largely on the natural sciences, are conceptually and morally bankrupt and need to be replaced by more sophisticated models."² And Sherry Ortner, an important cultural anthropologist, declares that "the Manichean struggle between 'materialism' and 'idealism,' [between] 'hard' and 'soft' approaches" as something that occupied academic attention in the decade of the sixties and early seventies, but which has now been superseded by an interpretive discourse of an entirely different character.³

Ethnohistorians and others can, of course, refight these essentially epistemological problems. The contest may bring fresh light to an old debate. But if we are to sally forth once again wearing this or that philosophic livery, we are obliged, I would think, to know our enemy.

Professor Moore's depiction of the state of history verges on parody. He takes a swipe, for example, at a group of historians who write for "the public." These authors produce "tracts...in which hero-worship and patriotism are still major if not always explicit themes." Historians, we learn, are all too eager to sell their intellectual souls for material reward. Such courtesan scholars at Moore's university can apparently win the favor of wealthy patrons by producing "complimentary" accounts "of the oil and cattle business." There are other equally bizarre and largely gratuitous generalizations scattered through Moore's essay. My point is not to defend historians against these charges--though I would hazard that if Moore had read the brilliantly crafted works of scholars such as Edmund S. Morgan, C. Vann Woodward, Lawrence Stone, or Bernard Bailyn, he might have tempered his remarks. Rather, by telling us of historians who have sold their intellectual souls to oil barons in search of hagiographers, Moore not only reduces the power of his own argument, but more important, threatens to close off a potentially valuable interdisciplinary conversation. Ridicule is certainly no way to win converts.

But such hyperbole aside, it is clear that Moore is prepared to lead the "materialist" charge against a host of foe, the mentalists and idealists, those who revel in subjectivity and opacity of thought, the practitioners of hermeneutic analysis, the mystics and poets amongst us. I suspect that Moore is not really prepared to argue a genuine materialist position, though there is no point in disputing labels. From my perspective, he seems enamored of what I would call a positivistic or empiricist philosophy. But by whatever name, his philosophy provides a foundation for scientific

history--apparently the clearest form of clear thinking. He urges us to formulate our questions in testable or falsifiable propositions, and though these propositions need not always lend themselves to quantitative analysis, they generally involve numbers. And like other positivists, Moore works out of paradigms borrowed from the natural sciences, and building upon small bits of "certain" knowledge, he attempts to construct theories.

Before examining the underpinnings of this "scientific history," I should note those points about which Moore and I agree. I do not advocate "unclear" thought, nor do I admire "reductionist" logic. Moreover, like Moore, I believe that scholars should read historic texts with care and imagination, teasing out nuances, spotting ironies, and exposing inconsistencies. His creative reading of documents produced by Indian agents is a model of interpretive finesse. Like Moore, I would insist that anthropologists and historians fully appreciate the power of computers to reveal significant patterns within aggregative data. And finally, we concur that showy mathematical manipulations are often not only pretentious, but also counterproductive in developing elegant analytic statements.

But however many assumptions about the production of new knowledge Professor Moore and I may share, considerable differences remain. I would like to explore these difficult points in some detail, but in these short comments, I must reluctantly concentrate my attention on three specific issues. Let me state in passing though that it is testimony to the excellence of Moore's essay that he has managed to challenge us on so many fronts. His claims for "scientific history" deserve careful scrutiny.

My first objection relates to Moore's counsel on the use of sources. He posits a link between the kinds of questions that we ask of the past and the kinds of evidence that we employ to answer those questions. This may sound unexceptionable. But remember that Moore wants us to ask "scientific" questions, questions that lend themselves to possible falsification through quantitative methods. And what sources would we entertain to meet this stipulation? The answer is presumably aggregate data that reveals something significant about broad patterns of human behavior. This approach, I would suggest, is tautological in character. We are urged to ask questions of those records that can yield the very sort of answers that we set out to discover in the first place. It does not matter greatly that such investigation occasionally produces surprises--a clustering that we did not anticipate or a distribution that seems anomalous, for example--since the original enterprise was set up within such a restrictive framework. As the British social historian Gareth Stedman Jones reminds us, we often learn something from such exercises "not because this information is vital to the solution of important historical questions, but simply because it lends itself more readily to quantification."⁴

This is potentially a more serious matter than it might at first appear. As anthropologists have repeatedly discovered, similar patterns of human behavior have been discovered throughout the world. Francis Hsu, for example, observed that the structure of Eskimo families was remarkably similar to that of the New England Yankees.⁵ To generate a theory of family structure out of such data would surely be a waste of effort. What we want

to learn is not more about the statistical incidence of certain forms, but rather, something about the meanings that the Eskimo and the Puritan took to these familial relations. It is such interpretive dead-ends that Clifford Geertz had in mind when he warned that "two events that may appear identical from a purely behavioral point of view may have radically different meanings and outcomes depending on the observer's interpretation of the intention of the person involved in the events."⁶

My second objection to Professor Moore's "scientific history" grows logically out of the first. In an effort to formulate questions which in his words are "vulnerable to testing by quantitative methods," Moore simply lops off too much that is fundamental to the human condition. His closing comments, for example, establishes an agenda for future research, a set of leading questions about "aboriginal social and political condition," about quantity and distribution and size. One's heart sinks as one reads Moore's list. Is this really where the field of American Indian history should be going? To be sure, his questions may be subject to "testing," but an uncharitable critic might observe that such studies sound a little boring. One wonders about other aspects of Native American life. About love and passion, ideas and values, meanings and interpretations, sentiments and ideology, in other words, about the very elements of past culture that gave emotional texture to the experience of everyday life.

This is the point that we should have learned from the debate over Time on the Cross: the Economics of American Negro Slavery, the famous--some would say infamous--book written by Robert Fogel and Stanley Engerman.⁷ As

you remember, these two respected scholars painstakingly reconstructed the lives of bondsmen living in the ante-bellum South. Employing extraordinary imaginative statistical methods, they established that slaves were reasonably well-fed, comparatively well-clothed, acceptably well-housed. And as their numbers revealed, not all that many black families were sold down the river to New Orleans. No one would dispute that this was "scientific" history. And of course, as reviewer after reviewer correctly explained, all the numbers in Time on the Cross did not tell us much about being a slave.⁸ How does one figure into the equation the meaning of un-freedom? How many children have to be separated from their parents through forced sales in order to generate statistically meaningful fear, anxiety, sorrow? I am not accusing Moore of insensitivity to the Native American experience. Far from it! I am only suggesting that his particular version of positivism leaves him--and those who follow his lead--open potentially to these kinds of grotesque misrepresentation of the ways that real men and women interpreted their lives.

My third reflection on Professor Moore's "materialism" is more philosophic in character, and I fear that there is not space fully to develop this line of criticism. Let me say simply that Moore's particular approach privileges a particular kind of inquiry, a particular way of interpreting "the world out there." And while the "scientific" approach may be perfectly adequate under certain circumstances, it is most assuredly not the only perspective that one might take. As Rorty and others have argued, there is not a world out there waiting to be discovered. Rather, human beings create categories, ways of ordering experience, that serve their

needs. The methods employed by the natural scientist to make truth claims are not philosophically preferable to those of the poet, or the historian, or the cultural anthropologist, only different. Philosopher Quentin Skinner has argued that "none of the empirical sciences has a privileged foundation in the sense of being able to claim a unique, essential grasp of reality. The empirical sciences all describe different realities, each with its own internal structure, as indeed do the humanities..."⁹

This may strike Moore as an unacceptably loose formulation. But he should remember that a multiplicity of perspectives, a diversity of ways of knowing, does not necessarily lead to a lack of intellectual rigor. The truths generated by the poet, by the historian, and by the chemist can be judged on grounds of internal consistency and coherence. And as Moore well knows, even such a revered scientific tool as the psi-function in quantum mechanics yields an apparently fuzzy representation of reality, a probabilistic range of answers. Why, if this is so, should we demand a greater level of exactitude from ethnohistory than physicists demand of systems analysis? To quote Lewis Carroll, the closer we get to the scientific paradigm, the "curiouser and curiouser" it becomes.

But even if one were to privilege Professor Moore's categories of analysis, one would still be confronted with the immense problem of moving from the present to a past--any past. How could one establish whether Moore's categories, those that inform his agenda for future research in Native American history, were also the categories of the people he purports to study? This is a serious conceptual problem, one that he attempts to

mask behind claims of empirical objectivity. A thing is a thing is a thing; or, is it? As Walker Percy, the philosopher-novelist, once said: "It is not enough to say that one is conscious of something. One is conscious of something as being something."¹⁰ The person who is busy construing the "facts" of a historical "reality," who discovers meanings in the endless computer printouts, is Moore--or as we learn from the examples he provides in the essay, one of his graduate students. Their twentieth-century categories may or may not be those of the nineteenth-century Plains Indians. Historian David Sacks has recently reminded us, "The facts that our sources contain simply do not speak for themselves....We make them intelligible, rather, by the questions we ask and the methods we use to answer them. In other words, what counts as fact remains bound to us and our ever-changing purposes."¹¹

Take the concept of a "nation," for example. Moore urges us to consider "how many nations were there in North America at any one time, and what do we mean by a nation?" Even if we could agree on what we mean by the term--a doubtful proposition--we would hardly have made any progress toward understanding what it meant to a Plains Indian. Surely such meanings change; they are situated in the rich particularity of experience. Considerations such as these compelled Hugh Seton-Watson to conclude in his Nations and States that "no `scientific definition` of a nation can be devised; yet the phenomenon has existed and exists."¹² It is at this point that Benedict Anderson's powerfully evocative phrase "imagined communities" is helpful. He insists that nations are "imagined" collectivities, mental projections across social space that link strangers in a web of common

identity. At different moments in time, people imagine their nation-ness in different ways. The category itself is open and fluid. "My point of departure," Anderson explains, "is that nationality, or, as one might prefer to put it in view of that word's multiple significations, nation-ness, as well as nationalism, are cultural artefact of a particular kind. To understand them properly we need to consider carefully how they have come into historical being, in what ways their meanings have changed over time, and why, today, they command such profound emotional legitimacy."¹³ My point is that I would like to know how a people in the past sorted out their experience before I projected my categories or meanings onto them.

Let me conclude on a positive (thought not a positivist) note. I am most emphatically not urging you to ignore what Professor Moore has to say. We should use quantification where it is appropriate. Moore provides us with an avenue to the past, a perspective, a set of possibilities, no more, no less. We have come too far to reject either scientific or hermeneutic analysis. A materialist approach that has not been leavened by culture is flat, even boring; a cultural approach that lacks methodological rigor is fantasy. Thus, we should be attempting to link these two modes of analysis, or if we do not succeed in yoking them, to at least to appreciate their equal philosophic standing. This is the kind of intellectual enterprise that William James had in mind when he wrote Varieties of Religious Experience, a tolerant conversation rather than a sermon to the converted. And it was in this spirit that I entitled these comments "varieties of clear thinking."

ENDNOTES:1

1. See, Richard Rorty, Philosophy and the Mirror of Nature (Princeton, 1979).
2. Louis A. Sass, "Anthropology's Native Problems: Revisionism in the Field," Harper's, 272 (May, 1986), p. 50.
3. Sherry B. Ortner, "Theory in Anthropology Since the Sixties," Comparative Studies in Society and History, 26(1984), p. 134.
4. Gareth Stedman Jones, "From Historical Sociology to Theoretical History," British Journal of Sociology, 27(1976), pp. 302-3.
5. Francis Hsu, "Structure, Function, Content and Process," American Anthropologist, 61(1959), pp. 790-805.
6. Sass, "Anthropology," p. 52.
7. Robert Fogel and Stanley L. Engerman, Time on the Cross (Boston, 1974).
8. Herbert G. Gutman, "The World of Two Cliometricians Made: A Review Essay," Journal of Negro History 60(1975), pp. 53-227.
9. Quentin Skinner, "End of Philosophy?" Review of Philosophy and the Mirror of Nature," New York Review of Books, 28(March 18, 1981), p. 48.
10. The quotation from Walker Percy can be found in Marshall Sahlins, Islands of History (Chicago, 1985), p. 146.
11. David Harris Sacks, "The Hedgehog and the Fox Revisited," Journal of Interdisciplinary History, 16(1985), pp. 267-80, quotation on 275. Also see, Paul Rock, "Some Problems of Interpretative Historiography," British Journal of Sociology, 27(1976), pp. 353-69; and Darrett B. Rutman, "Encounter With Ethnography: A Review of Isaac's Transformation of Virginia," Historical Methods, 16(1983), pp. 82-6.
12. Hugh Seton-Watson, Nations and States: An Enquiry into the Origins of Nations and the Politics of Nationalism (Boulder, Co., 1977), p. 5.
13. Benedict Anderson, Imagined Communities: Reflections on the Origin and Spread of Nationalism (London, 1983), pp. 13-14.

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CENSUSES AND AMERICAN INDIAN FAMILIES:
THE CASE OF WHITE EARTH RESERVATION, MINNESOTA, 1890-1920

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Censuses and American Indian Families:
The Case of White Earth Reservation, Minnesota, 1890-1920

Introduction

Why study American Indian families? My concern with "community" and social change molded my interest in Chippewa families as I formulated my plans to undertake a "community study" of a reservation population.¹ Therefore, this study does not touch upon many issues that would be of interest in a comparative study of the historic structures of American Indian families. This is as it should be. Mindless manipulation of data and statistics produces only meaningless measures. Clear research issues and questions should direct quantitative research.

I had as my goal to complete a more internal social history with emphasis on intra-reservation patterns of affiliations. I planned to bring Indian historical actors to center stage and focus on the initiatives and responses made by the White Earth people during the intrusive allotment/forced assimilation period of U.S. Indian policy in the late 19th and early 20th centuries. The complexity of federal policy coupled with the creativity of Indians' adaptations forced me to develop a more holistic view of

the period where I came to emphasize economic change and political factionalism as well.

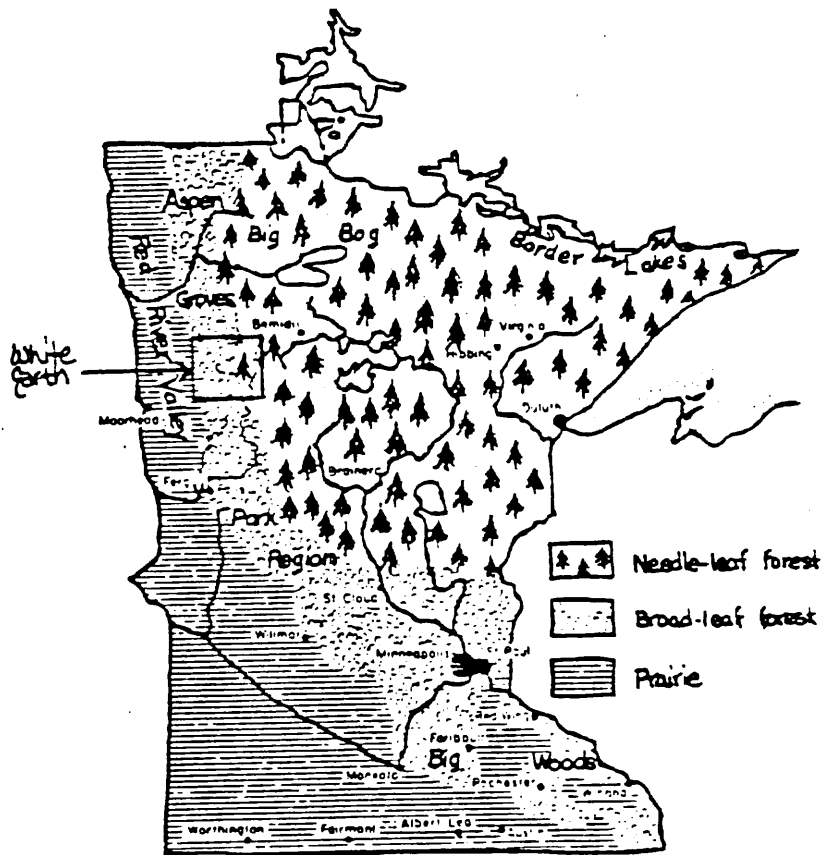
Ethnic or cultural divisions characterized the White Earth Indian population as more market-oriented families descended from Great Lakes fur trade/metis society joined more traditional Indians from interior Minnesota bands. These ethnic differences marked the genesis of community relationships at White Earth. Settlement patterns, social and religious affiliations, political ties, household sizes and surname frequencies all reflect a spectrum of intra-reservation band differences. Different ethnic world views, values and ethics erupted into a virulent political factionalism as Reservation residents developed conflicting views on the management of tribal resources. Eventually, the imposition of elective Reservation government created a permanent traditional minority as those with larger households and, perhaps, higher fertility rates gained political ascendancy. This essay will explore the social bases of intra-reservation differences primarily through simple quantification of serial sources that are organized by household or family groupings.

White Earth as a Subject

White Earth Reservation was created in 1867 amidst high expectations. Straddling Minnesota's prairie-forest transition zone, White Earth contained diverse

and abundant resources, plentiful water and was located close to nearby markets. Agriculture would thrive on the western prairies and the sale of the timber in the eastern forests would fuel the assimilation programs of the U.S. government...or so the logic went. The forests, streams, lakes and especially the marshes assured that the Indians might continue their traditional seasonal harvesting of fish, game, wild rice and maple sugar, as they had in the past. At its inception White Earth's ecozones supported diverse options for securing a livelihood (See Map 1).²

The 1867 Treaty and the 1889 Nelson Act both furthered the U.S. government's agrarian assimilation plans for the Chippewa. In keeping with national policy, both documents mandated the allotment of the Reservation land base as a prominent feature. U.S. Indian policy intended nothing less than the wholesale replacement of Indian cultures with a romantic version of the American agrarian ideal, the small-scale, independent yeoman farmer. Policymakers reasoned that the allotment of the land base and education in American mores and the principles of market behavior would foster an appreciation of private property among Indians with collective values. Treaties and legislative terms provided for agricultural accoutrements, houses, schools and periodic distributions of tribal funds to encourage the transformation of Indians' values. The U.S. government would hold allotted lands in trust for 25 years to protect naive Indians



MAP I Vegetal Zones in Minnesota

*From Harold Hickerson, "The Virginia Deer and Intertribal Buffer Zones in the Upper Mississippi Valley." In Man, Culture, and Animals: The Role of Animals in Human Ecological Adjustments. Eds. Anthony Leeds and Andrew Vayda. Publication 78, American Association for the Advancement of Science, Washington DC, 1968.

from unscrupulous outside corporations and settlers. If U.S. assimilation policy had a chance to succeed anywhere, White Earth should have been a showcase.³

The abundance of documentation pertaining to White Earth presented almost optimum research conditions. In addition to such standard historical sources as personal accounts, government documents, correspondence, and ethnographic materials, there are maps, transcripts of court testimony, sources generated by Indians themselves, oral histories and, most importantly for the purposes of this essay, the serial records of the Bureau of Indian Affairs (BIA). The diverse forms of documentation combined with interdisciplinary research techniques allowed for cross-checking to ensure greater reliability. This type of context is essential for meaningfully interpreting statistics.

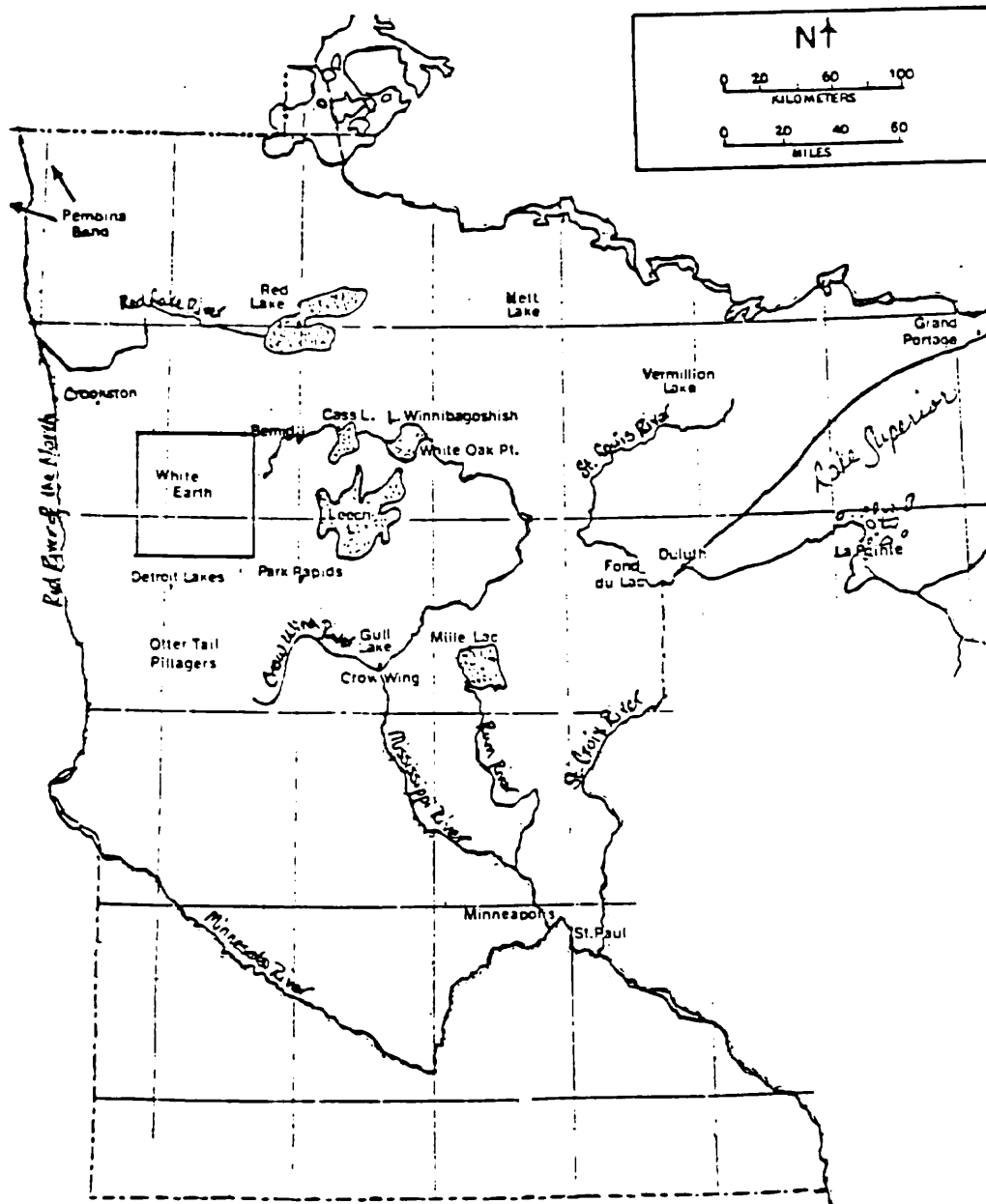
The combination of White Earth's historical importance and the volume of available research materials allowed me to pursue a "community study" of the Indian population. Indian people become the primary historical actors when we ask what forms families and communities took at the Reservation. Did Chippewa households reflect the persistence of more traditional social patterns, or were they creative adaptations? How did intra-reservation social affiliations affect political relationships? How did the White Earth Indians adapt to this period of massive exploitation of resources by external corporate interests? These questions move us away from a mere

recitation of agents and government policy and bring us as close as we can presently get to individual Indian behavior.

Origins of the White Earth Population

Three major chain-migrations brought various aggregations of Chippewa families and bands together at White Earth in the late 19th century. The Gull Lake Mississippi band and "mixed blood" ex-traders and merchants and their offspring from the Crow Wing trade entrepot migrated following the creation of the Reservation in 1868. By 1875 800 had migrated. In 1876, 2 bands, the Otter Tail Pillagers and the Pembina band, also relocated at White Earth raising the total population to 1,427 (See Map 2). Each of these bands settled in a different area of the Reservation that suited its cultural orientation.⁴ The lack of serial documents or censuses makes it difficult to determine much internal social detail before 1885 when the Indian Office began producing annual censuses.⁵

The third major migration stream began after the passage of the 1889 Nelson Act, which implemented the 1887 General Allotment Act among the Chippewa Indians of Minnesota. Through the Nelson Act, policymakers attempted to concentrate all of the various Chippewa bands in Minnesota at White Earth. The removal provisions, however, were made voluntary, producing migrations



MAP 2: ORIGINS OF WHITE EARTH BANDS*

*By Melissa L. Meyer, using a Denoyer-Geppert base map.

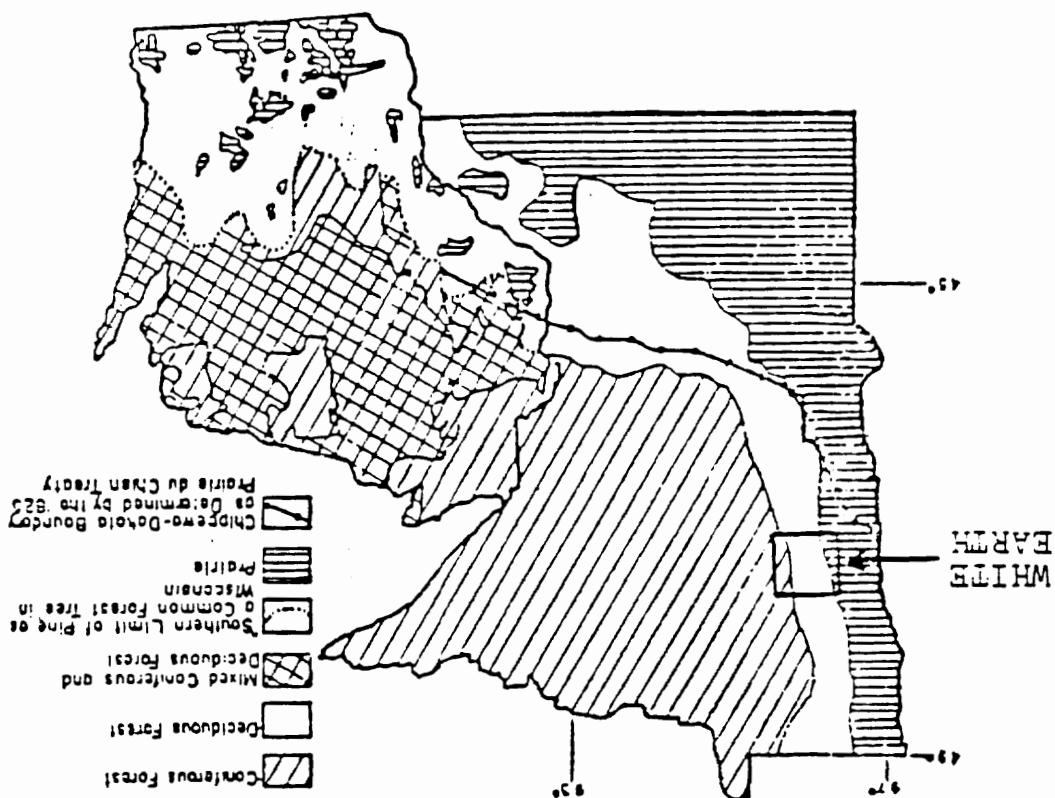
from the northern Minnesota reservations that spanned a 15 year period. Increasingly after 1890, emigrants from the interior Minnesota reservations of Mille Lacs, Leech Lake, Lake Winnibagoshish, Cass Lake, Fond du Lac and Grand Portage joined the resident White Earth Indians who had migrated earlier (See Map 2).⁶

Over the years, the opportunities available at White Earth enticed diverse immigrants. More traditional Indians who faced decreasing game populations sought to integrate subsistence farming into their seasonal round by moving to the relative abundance of White Earth's prairie-forest transition zone. Due to its location in an old buffer zone between the Dakota and Chippewa, extensive human habitation had not exhausted White Earth's resources as it had around the forested areas of Leech Lake and Gull Lake (See Map 3). By selling both native and cultivated produce on the market and working for wages, the White Earth Chippewa replaced the fur trade connection through which they had received manufactured goods without much social or economic dislocation.⁷

The provisions for land and services also attracted "mixed blood" individuals scattered throughout northern Minnesota who faced diminishing economic opportunities. Great Lakes fur trade/metis society spawned this class of bilingual, bicultural brokers who relied on their genetic ties to the Chippewa to gain recognition for their claims to benefits at White Earth. The denouement

*From Harold Hickerson, The Southwestern Chippewa: An Ethnohistorical Study. New York: Kraus Reprint Co., 1974: 28.

MAP 3: THE PRAIRIE DU CHIEN TREATY
IN THE PERSPECTIVE OF VEGETAL ZONES*



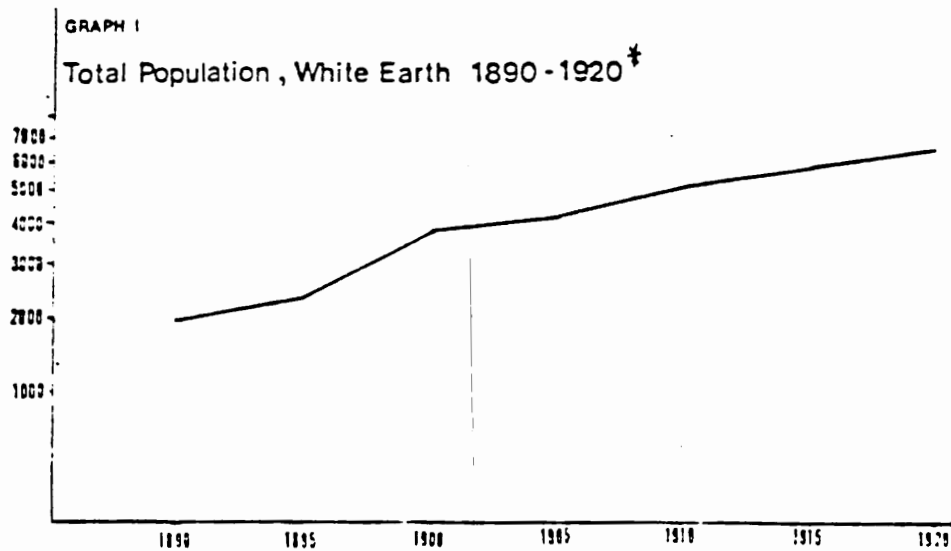
of the western Great Lakes fur trade displaced these individuals who brought their market behavior with them to White Earth in search of a renewed economic niche.⁸

The diversity of this entirely immigrant population made it clear that community genesis and intra-reservation differences would be significant social issues to explore. Early on, community relationships developed in local, geographic settlements that reflected previous band ties. However, all band members did not settle together which fostered a comingling of those who shared similar lifeways that reflected White Earth's ecozones. Although band ties influenced settlement patterns, cultural or ethnic differences were the most significant divisions on the Reservation.

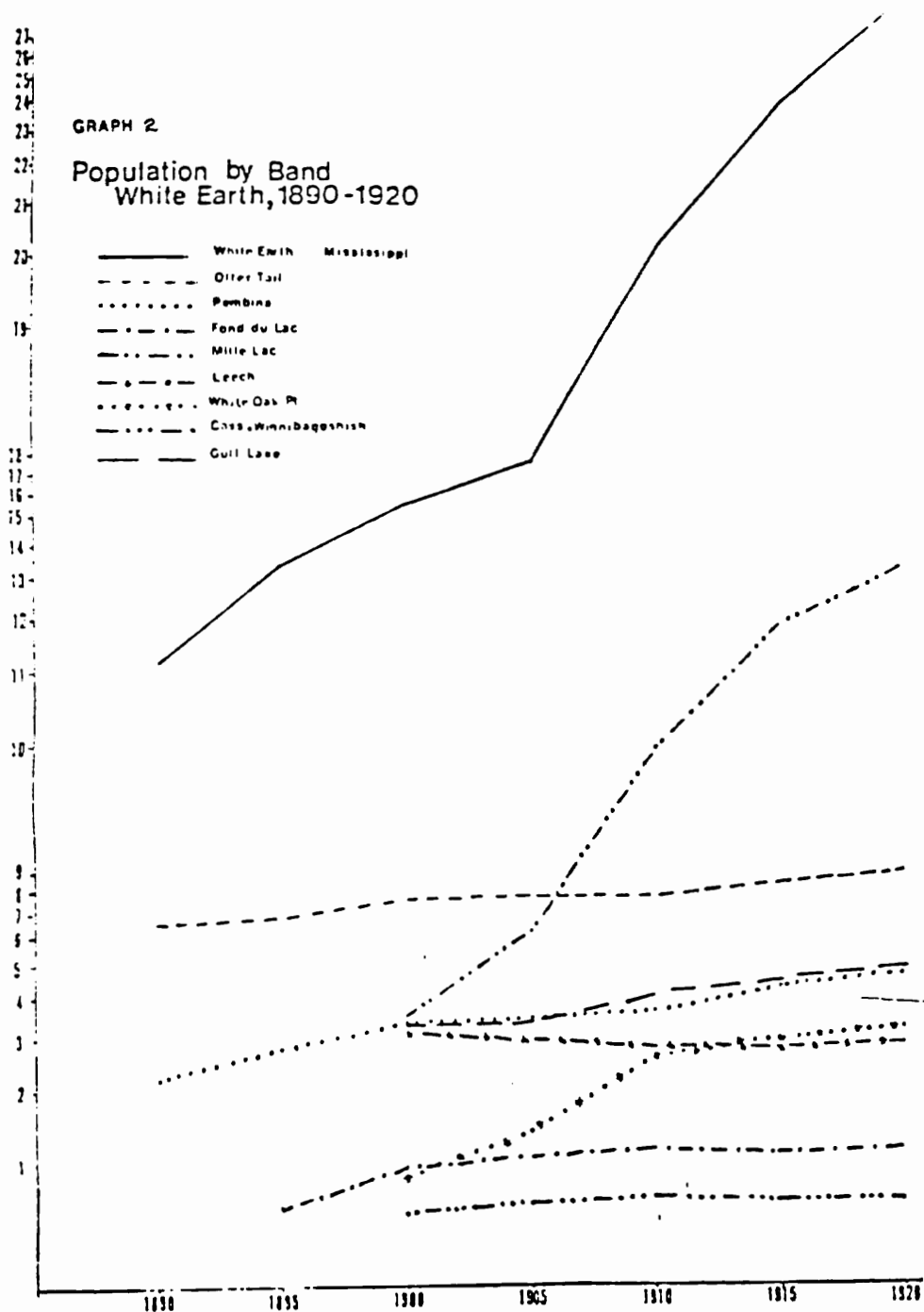
Prior to the removal of the trust relationship from "mixed bloods'" allotments, a low population and relatively abundant resources smoothed intra-reservation affiliations. However, rampant land fraud caused ethnic differences to erupt into a bitter political factionalism that generated reams of documentation. A steadily rising population exacerbated these cultural cleavages (See Graphs 1 and 2). Standard literary sources reinforced my search for the social relationships that might account for the factional discord.⁹

Evaluation of Bureau of Indian Affairs Censuses

Given these issues, Bureau of Indian Affairs "censuses" for White Earth have special utility because they are



*Compiled from BIA Censuses, White Earth Reservation, 1890-1920, at 5 year intervals.



*Compiled from BIA Censuses, White Earth Reservation, 1890-1920, at 5 year intervals.

organized by band and enumerate individuals, ages, gender, modified households, and the relationship of each individual to the head of household. Politically, many individual White Earth residents organized along band lines and pursued special interests stemming from treaty rights. No other form of serial documentation records band differences, making these "censuses" crucial for exploring social relationships.

However, closer evaluation of these demographic sources revealed that the "censuses" were actually enrollment lists that recorded those entitled to rights as White Earth band members, not actual Reservation residence. Strange enumeration procedures initially caused me to question the composition of these "censuses." Unusual strings of solitary individuals skewed measures of average household size, producing a mean of one. Many examples of single parents labelled as "wife" or "husband" with no spouse listed led me to believe the document recorded band enrollment. Marriages between bands probably produced this listing of odd households. When I encountered the names of individuals whom I knew lived off-reservation, I began to take my hunch more seriously.

No "instructions" explain these censuses and no correspondence explains the enumeration procedure. There are no reports of visits to Indian homes and no directives issued by the Commissioner of Indian Affairs to commence the compilations. Considering the voluminous amount of documentation relating to the White Earth

Reservation, this lack of bureaucratic attention seems doubly strange.

I suspect that the agent compiled the listing in the agency office annually from cards that he periodically updated.¹⁰ Marriages, births and deaths required changes in the original numbers assigned to individuals which were recorded at the end of each band listing. Off-reservation residents appeared as strings of individuals who presumably would serve as the stem for a new family grouping of off-reservation enrollees. Criteria for enrollment changed over time. These enumeration patterns help to account for the skewed mean household size of 1. Rather than listing Reservation residents, the BIA "censuses" actually enumerated all those officially recognized by the federal government as members of the "White Earth Band" of the "Chippewa Indians of Minnesota."

The implications of this discovery for aggregate population totals change over time; the censuses are more reliable earlier as they recorded immigrants and less reliable later as out-migration increased. The enrollment lists cannot produce reliable aggregate statistics with comparative, cross-cultural value, but they can still illuminate intra-reservation band differences because the problems affect all bands alike. If solitary individuals are omitted from the sample, a predictable spectrum of ethnic band differences emerges.

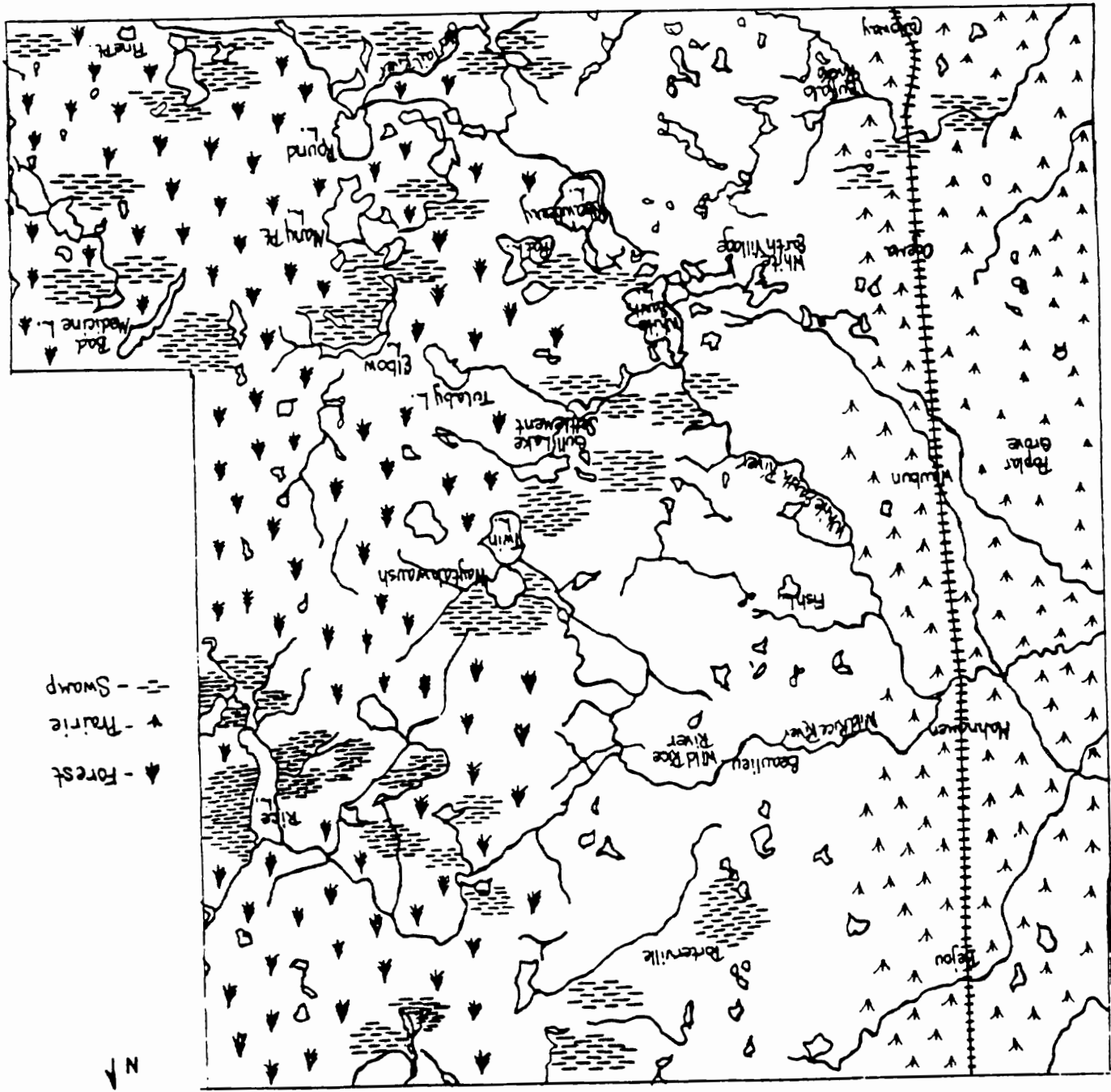
The Spectrum of Band Differences

At the passage of the 1889 Nelson Act, those who had migrated to White Earth over the past 2 decades had evolved a society based on ethnic affiliations, band ties and residence patterns. An east-west settlement pattern emerged as those who were drawn to market farming and mediating activities settled in the western prairies and parkland transition zone, while those who followed a more traditional seasonal round located in the eastern forest and lake country. These geographic divisions reflected both band ties and cultural preferences (See Map 4).

Nelson Act immigrants integrated themselves into this existent social order. Immigrants tended to maintain band ties, social roles and leadership patterns after their removal to White Earth. Drawn largely from more traditional interior Minnesota bands, most opted to join family and kin in the settlements of the eastern forests.¹¹

The life patterns of the traditional Chippewa carried them in different directions than the "mixed blood" innovators. Members of the 2 ethnic groups had only fleeting associations. Residence patterns separated them, and they tended to socialize and marry largely within their own distinct groups.¹² Traditional Chippewa generally practiced the Midewiwin religion or attended Episcopalian mission services, while those of French-Canadian descent tended to be Catholic. Their children

White Earth Indian Reservation



received different types of education. Mixed blood children more often received training at regional and national boarding schools and at boarding and day schools on the Reservation. Investigators frequently complained that students with very little Indian blood filled the schools, excluding many "full blood" children. Thus, in many spheres, the paths of the 2 cultural groups at White Earth crossed only infrequently.¹³

Although they made reference to genetic characteristics, Indian understandings of the terms "mixed blood" and "full blood" centered on cultural attributes. They differentiated between the symbols by referring to hairstyle, clothing style, type of house, cultural practices and, most importantly, economic ethics. "Full bloods" were consistently described as "poor" Indians who were concerned only with their subsistence and the equitable distribution of resources, while "mixed bloods" were "shrewd," understood the operations of the market economy, and accumulated material wealth. Prior to the involvement of key "mixed blood" leaders in land fraud, the term "mixed blood" carried little of the pejorative connotations that it later came to bear. Only U.S. policymakers interpreted the terms genetically. Indian definitions revolved around cultural characteristics that eventually evolved to include a political dimension.¹⁴

The concentration of Chippewa bands at White Earth created a fragmented collection of individuals accustomed

to different lifeways - a community of communities, so to speak. The geography and ecology of human settlement patterns reflected these cultural differences. Towns and villages in the eastern forests came to symbolize the traditional way of life still centered on the seasons. The mediating and entrepreneurial activities of the brokers at White Earth Village and successful commercial farmers in the west formed yet another cultural pattern, more tied to the market. Although many White Earth residents perceived these distinctions themselves, the ethnic divisions were not hard and fast and individuals easily deviated from the general pattern to create unique adaptations (See Map 4).

If the labels "mixed blood" and "full blood" accurately identify basic cultural differences between White Earth residents, the behavior of individuals ought to reflect this. The relative proportion of French or English surnames as opposed to Chippewa names can serve as a very rough indicator of the cultural orientation of each band (See Chart 1). Anglicized names in this context indicate a more innovative choice of names, while Indian names reflect more traditional preferences. They also reflect the degree of inter-marriage with Euroamericans.¹⁵

The breakdown of bands at White Earth by surnames reflects differences in the duration and intensity of their involvement in Great Lakes fur trade/metis society. Centered at a long-established fur trade depot on the St. Louis River, the Fond du Lac band stood at one end

CHART 1: ANISHINAABE VS. ANGLICIZED NAMES, 1890-1920

Band	1890			1900				1910				1920			
	Ind	Eng	N	Ind	Both	Eng	N	Ind	Both	Eng	N	Ind	Both	Eng	N
Fond du Lac				3	3	93	91	2		98	111	.9	.9	98	113
Pembina	30	70	218	11		89	318	5	.3	95	361	2	3	95	472
White Earth															
Mississippi	25	75	1107	14	.7	86	1544	5	3	92	1995	2	3	95	2764
Gull Lake				28	.3	71	336	12	3	85	401	6	7	88	469
White Oak Pt.				72	3	25	88	30	9	61	259	13	13	74	315
Cass & Winnibagoshish				57	6	37	51	16	6	78	63	16	18	66	61
Otter Tail	84	16	652	70		30	741	42	10	48	744	16	22	62	886
Mille Lac				50		50	323	45	8	47	990	24	19	57	1308
Leech Lake				75	2	23	309	43	15	42	277	19	29	52	281

*Compiled from BIA Censuses, White Earth Reservation, 1890-1920, at 10 year intervals

of the spectrum of surnames. The Pembina band, an off-shoot from the Turtle Mountain Reservation in North Dakota and closely affiliated with the Red River Colony in Canada, exhibited characteristics similar to the Fond du Lac band. The White Earth Mississippi band regularly placed third after these 2 bands in the proportion of anglicized surnames. The Mississippi band at White Earth consisted of an amalgamation of social groups. In the earliest migrations to White Earth a large number of "mixed blood" descendants of fur traders from Crow Wing and Lake Superior and Wisconsin bands of Chippewa joined the more traditional Mississippi band centered at Gull Lake. As such, both patterns were reflected in the White Earth Mississippi band. However, the "mixed bloods" predominated, causing the characteristics of the Mississippi band to resemble more closely those of the Pembina and Fond du Lac bands; they shared common origins in fur trade society (See Chart 1).¹⁶

Traditional Chippewa bands retained their Indian names longer. Although the overall trend was toward increasing anglicization of names, the Leech Lake, Otter Tail and Mille Lacs bands maintained higher proportions of Indian names longer. In the past, their seasonal existence at interior Minnesota lakes kept them more isolated from Great Lakes fur trade society. While they participated in the trade, intermarriage had occurred on a more limited scale than among bands located near major trade entrepôts like Crow Wing,

La Pointe and the Red River Colony.¹⁷

These bands had the reputation of being more traditional and resided more in the eastern forest and lake country at White Earth. When they adopted surnames, they more often anglicized Chippewa names, rather than using standard French or English choices. Surnames with Chippewa origins might be either literal or phonetic translations of Indian names. Examples of names translated literally are: Drumbeater, Fineday, Badboy, Hole-in-the-Day, Skip in the Day and Whitefish. Examples of phonetic translations are: Anywaush, Wadena, Gahbow,

and Skinaway. Interior Minnesota bands adhered to traditional ways longer and this was reflected in both their retention of Chippewa names and in their choice of anglicized surnames.¹⁸

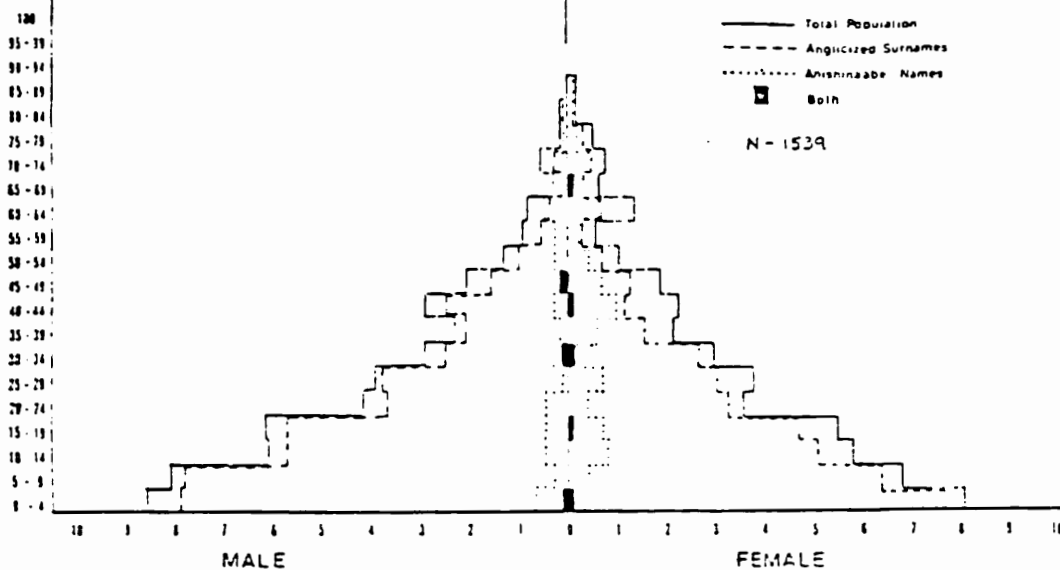
The patterning of bands within this spectrum of surname frequencies does not represent a concrete categorization of bands by cultural characteristics. Several bands lie in a middle zone and exhibit some individual variation. However, specific bands continually occupied the upper and lower limits, establishing the pattern. The Fond du Lac, Pembina and Mississippi bands continually exhibited higher proportions of French and English surnames, while the Otter Tail, Mille Lacs and Leech Lake bands consistently revealed higher proportions of Indian names. The patterning of surnames at White Earth reflected cultural differences between bands.

Age pyramids showing the distribution of surnames illustrate the anglicization process (See Graphs 3, 4, 5 and 6). In 1890 the White Earth Mississippi band showed a 75% majority of French and English surnames. Only a small, shrinking core of Indian names remained. By 1920, Chippewa names had all but disappeared. Some older residents took their Indian names with them to the grave, but fewer children identified themselves primarily by their Chippewa names. The pattern of surnames in the Otter Tail band replicated this larger trend, but reveals an earlier stage of the process. Beginning in 1890 with 84% Indian names, they retained only 38% by 1920. These examples demonstrate how bands differed in their adaptations to the transitional way of life that steadily displaced tradition at White Earth.¹⁹

The average size of households also varied among bands at White Earth. The spectrum of household sizes at White Earth corresponds in its basic configurations to the spectrum of surname frequencies (See Chart 2). With some annual variation, the diagnostic bands at the upper and lower ends of the spectrum remain the same. Bands with a longer history of participation in Great Lakes fur trade/metis society tended to have larger households while those from more traditional Minnesota bands tended to have smaller households. Cultural differences between bands were reflected in the Reservation social structure.²⁰

GRAPH 3

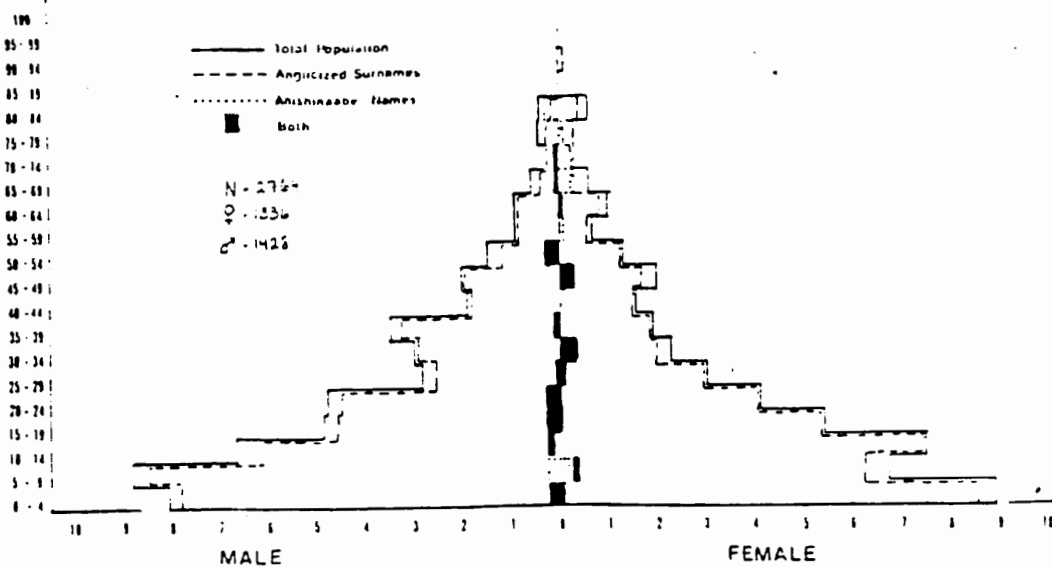
Age Structure and Patterns of Names: Mississippi Band, 1900



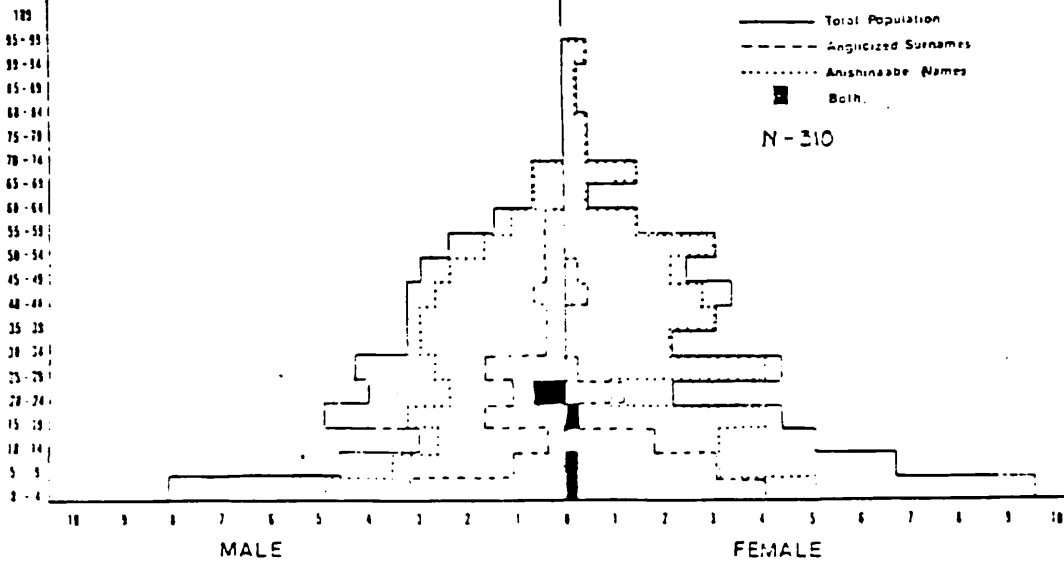
*The following graphs were compiled from BIA Censuses, White Earth Reservation, 1900 & 1920.

GRAPH 4

Age Structure and Patterns of Names: Mississippi Band, 1920



GRAPH 5
Age Structure and Patterns of Names: Leech Lake Removals, 1900



GRAPH 6
Age Structure and Patterns of Names: Leech Lake Removals, 1920

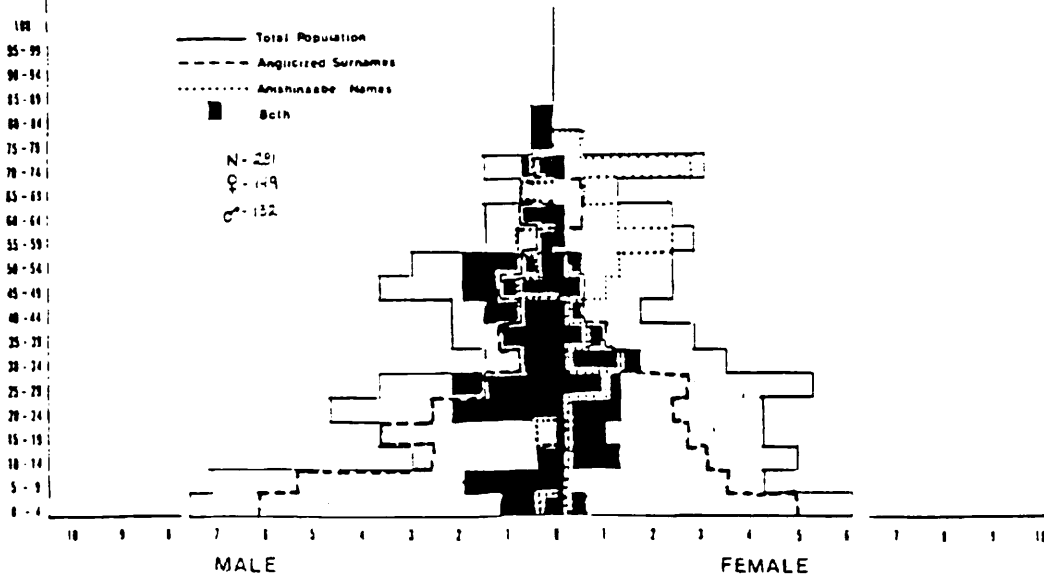


CHART 2 : MEAN HOUSEHOLD SIZE, BY BAND, MINUS
SOLITARY INDIVIDUALS, WHITE EARTH
RESERVATION, 1890-1920*

Band	1890	1900	1910	1920
Fond du Lac		5.3	5.3	7.5
Pembina	4.9	5.7	6	7
White Earth				
Mississippi	3.8	5	5	5
Gull Lake		5.1	4.6	5.3
White Oak Pt.		4.6	4.5	5.4
Cass & Winnibagoshish		4.6	6.3	7.6
Otter Tail				
Pillagers	4.2	4.4	4.2	4.8
Mille Lac		5.1	4.9	4.7
Leech Lake		4.4	3.7	4.9

*Compiled from BIA Censuses, White Earth Reservation,
1890-1920, at 10 year intervals.

None of these rough measures can be considered reliable standing alone. However, the recurrence of established patterns through several measures enhances their reliability. The BIA "censuses" for White Earth cannot be used to construct reliable age structures in the years after about 1910, but they can help to establish patterned inter-band differences. When information derived in this fashion complements interpretations drawn from literary sources, the reliability of both is strengthened.

Linkage of Serial Documents

On the whole, measures of household size and surname patterns merely point the way for further analysis. Measures of household composition, marital patterns and a comparison of settlement and allotment patterns might increase our understanding of inter-band relationships. BIA "censuses" as enrollment lists, are unreliable for this purpose. The 1900 and 1910 U.S. federal manuscript censuses might prove more useful for this, but they are enumerated by residence, which will obscure band distinctions.²¹

The key lies in the computer-assisted linkage of serial documents (See Figures 1, 2 and 3). By linking individuals between censuses and perhaps with other serial sources, the enumerated categories from all the documents can be integrated. The potential exists to gather a wealth of information at the individual

FIGURE 1: Categories of Information Contained on the 1900 & 1910
U.S. Federal Manuscript Censuses

General Population Form

Location

-In Cities: Street

House #

-# of Dwelling - Order of Visit

-# of Family - Order of Visit

Name (Include all persons in family. Omit children born since
June 1, 1900)

Relationship to Head of Family

Race

Sex

Date of Birth: Month

Year

Age

Marital Status

of Years Married

Mother of # of Children

of Children Living

Place of Birth

Father's Place of Birth

Mother's Place of Birth

Year of Immigration to U.S. (significant when close to
international borders)

of Years in U.S.

Naturalization

Occupation

Months not Employed

Attended School (in months)

Can Read?

Can Write?

Speaks English?

Dwelling Information: Owned or Rented?

Free or Mortgaged?

Farm or House?

of Farm Schedule

Special Inquiries Relating to Indians

Other Name (if any)

Tribe

Father's Tribe

Mother's Tribe

Degree of White Blood (0, $\frac{1}{2}$, $\frac{1}{4}$,)

Polygamous (if married)

Taxed?

Year Acquiring Citizenship

Citizenship Acquired through Allotment?

Fixed or Movable Dwelling?

FIGURE 2: Categories of Information Contained on the
Bureau of Indian Affairs "Censuses", 1890-1920

Band Affiliation
on Census
Indian Name
English Name
Relationship to Head of Household
Age
Sex
(notations about changes in numbers and reclassification
of individuals)

FIGURE 3: Categories of Information Contained on the
1920 Blood Roll

on List
English Name
Indian Name
Date of Birth
Sex
Blood Quantum
Original Allotment Selection
Additional Allotment Selection(s) > by township and range lines
(in alphabetical order)

level that will cause Indian groups to emerge as some of the best-documented people in the world.²²

Such techniques have the potential for opening numerous avenues of inquiry. Most significant for my study of the social relations of the White Earth Indians are measures of household composition, marital patterns, residence patterns in relation to allotment patterns and patterns of out-migration. I expect to find further evidence that supports my earlier portrayal of White Earth society. The methods will be intended to serve as prototypes to illustrate the potential of these documents.²³

Examining marital patterns and household composition may provide insights into the life cycles of White Earth families. The bilateral band organization of Chippewa society in the last half of the 19th century had evolved in tandem with the need for social flexibility. The bands splintered and formed as the conditions of life dictated. This flexibility aided more traditional Chippewa in their adaptation to reservation life at White Earth. Nuclear family forms probably characterized ex-traders and their metis offspring, who modeled their choices after the dominant society, with modifications.

Rather than interpreting the preponderance of nuclear families at White Earth as evidence of "modernization," they must be evaluated with the diversity of the White Earth population in mind. Some nuclear families may be illustrative of phases of a traditional Chippewa

life cycle embodied in the bilateral band. Simple family households can be accommodated within this framework. Young couples might expect older relations, especially parents, eventually to reside with them. Conversely, some extended families may have been temporary arrangements. Families extended downward might anticipate losing younger couples at some point. In the early years at White Earth, laterally-extended households might reflect the youthfulness of the immigrant population and the pattern of siblings turning to each other for support during a period of uncertainty. We may never know which norms and values were the operative agents in these situations, but it is better to recognize the wide variety of possible interpretations than to opt for simplistic models of disintegration or persistence.²⁴

While the 1900 and 1910 U.S. federal manuscript censuses detail household members in relation to the head of household, and thus capture marital relationships, they do not categorize these by band. Such aggregate data cannot approximate the diversity of the White Earth Indian population. They must be linked with the BIA "censuses" to reveal this dimension. If this linkage can be accomplished for at least 2 points in time, insights pertaining to many questions may come within reach. Do marital patterns reflect the spectrum of band differences? Patterns of band endogamy and exogamy may reveal boundary-maintaining mechanisms. Were certain

bands more likely to marry out of the Indian population? Changes in these patterns may be discerned by comparing such measures over time. From this perspective, marital choices become a form of adaptive behavior which can either cement or dissipate local community ties. I suspect that intermarriage diluted band ties over the years and contributed to the increasing ascendancy of cultural or ethnic divisions between people.

Linking these censuses may also permit a comparison of residence and allotment patterns. Federal censuses provide the dimension of actual residence, BIA "censuses," again, supply band affiliations, and the 1920 Blood Roll lists allotment selections for each individual.²⁵ Actual computer mapping becomes a part of the research technique in this case. Do residence and allotment patterns reflect the spectrum of band differences? Did Indians actually move onto their assigned allotments of land, and if so, how long did they remain? Did members of more market-oriented bands select more valuable agricultural or timbered allotments? Investigations of land fraud claims suggest such a pattern.

Combining these censuses may also shed light on migration patterns that have always confounded scholars. Here, the real value of the BIA "censuses" as enrollment lists becomes apparent. Deriving the resident population from the 1900 and 1910 federal censuses and then subtracting the total from those listed in the BIA "censuses" should provide a rough measure of out-migration by band. The federal censuses list the Indian population

at the end of each enumeration district which will permit the construction of the distribution of the Indian population for the State of Minnesota. Not all of these off-reservation Indians will be White Earth emigrants, but the measures will serve as an indicator of the timing and distribution of Indian migration streams. Aggregate numbers provided by the 1920 and 1930 federal censuses may permit trends to be traced over an even longer time period.

By combining quantitative methods with oral history and traditional documentary research, I hope to show how the alienation of resources through allotment and sale after 1907 fostered the out-migration of both ethnic groups on the Reservation. This process transformed families and communities at White Earth. The age structure of the Reservation Indian population should reflect this. We might anticipate that young adults in the prime of life (ages 20-40) more often left the Reservation in search of employment, leaving behind a larger proportion of older people, young children and, perhaps, women. I expect to find that these out-migration streams produced new urban Indian communities, spawned back and forth patterns of urban/reservation visiting and spun-off some individuals who abandoned Reservation ties for other social relationships. The more intimate, close-knit, geographically-based communities at White Earth shrank in size and bent to accommodate these evolving migration patterns.²⁶

Conclusion

The quantification of serial data has aided in illuminating the social bases of intra-reservation differences at White Earth to which more traditional documents only alluded. The results have proven useful on a number of levels and also suggest avenues for future research.

I originally intended to pursue a reservation "community study" during the allotment/forced assimilation period of U.S. Indian policy. In purely descriptive terms, information on social patterns and band differences can begin to explain how Indian people adapted during the U.S. government's most intrusive attempts to remake their cultures. The diversity of White Earth's ecozones supported the lifeways of both the market-oriented contingent that more closely paralleled the ways of the dominant society as well as the more traditional element that persisted in its seasonal orientation. The measures taken underscore the need to see White Earth's population as a diverse assemblage of people who exhibited quite different patterns of behavior. Scholars know so little about the social history of Indian communities during this time period that descriptive, compensatory history has its merits.

The discovery of these ethnic cleavages helps to explain the emergence of virulent political factions

at White Earth. In 1978, Robert Berkhofer observed that historians know relatively little about the cultural bases of such factions - whether they broke down according to "old politics of ins and outs, patronage distribution, and family affiliation."²⁷ Almost a decade later, his appraisal still applies. Uncovering the bases of such factional affiliations requires more than reference to participant-observers' accounts. The exploitation of available serial data can add an empirical element to impressionistic, narrative sources. Ideally, both types of data will reinforce each other, as they have at White Earth. A future project that links the 1900 and 1910 U.S. federal manuscript censuses with BIA "censuses" and the 1920 Blood Roll should provide further detail that reinforces and expands my earlier analysis.

The results of this preliminary analysis suggest the utility of a future study that explores migration away from the Reservation. Corporate interests in the form of banks, speculators and lumber companies rapidly acquired most of the allotted lands at White Earth after special legislation removed the trust status of "mixed bloods'" lands in 1907. In effect, unimpeded industrial capitalism undermined the contrasting adaptations made by both ethnic groups. Increasing out-migration and dependence on government gratuities followed, with exceptions, for all of the White Earth Chippewa, "mixed blood" and "full blood" alike. In this case, dependency theory and the metropolis-

satellite model have bearing.²⁸

Referring to more general theories that have applicability beyond the narrow sphere of American Indian history will generate comparative data and help integrate Indians' experiences into the broader fields of U.S. and world history. Including reservation enclaves and urban Indian populations can contribute to the growing interest in the study of communities in American history. As the nation's only indigenous people, the inclusion of Indians in this dialogue can help refine theories in this nascent area. As scholars we must be aware of these broader theories so that our analyses can reinforce or modify them. In this way, we can help to reduce the provincialism that afflicts the field of American Indian history. Embracing quantitative methods can help take us in this direction, but, alone, they are incapable of providing the breadth of information produced by the interdisciplinary use of a wide variety of sources and methods.

ENDNOTES

1 I favor a processual approach to the study of "community" that emphasizes the varying forms that community can take. Such an approach can move us away from a simplistic, progressive portrayal of the breakdown of idyllic, geographically-bounded communities due to a constellation of processes amorphaously-labelled "modernization." We can then identify the causes of community transformation and patterns in this process. Thomas Bender in Community and Social Change in America (New Brunswick: Rutgers University Press, 1978) discusses the theoretical vacuity that plagues many community studies, but offers little direction for correcting their inadequacies. In "Families, Friends and Neighbors: The Structure of Community in Schenectady, New York, 1660-1790," Forthcoming PhD Dissertation, University of Minnesota, Edward H. Tebbenhoff offers a discussion of network analysis that will help refine research on the history of communities.

2 For further information on Minnesota's prairie-forest transition zone, see: Herb E. Wright, Jr., "Late Quaternary Vegetational History of North America," in The Late Cenozoic Glacial Ages, ed. K.K. Turekian (New Haven: Yale University Press, 1971), 425-464; P.K. Simms and G.B. Morey, eds., Geology of Minnesota: A Centennial Volume (St. Paul: Minnesota Geological Survey, 1972), 515-547, 561-580; John R. Borchert, Minnesota's Changing Geography (Minneapolis: University of Minnesota Press, 1959); Harold Hickerson, "The Virginia Deer and Intertribal Buffer Zones in the Upper Mississippi Valley," in Man, Culture and Animals: The Role of Animals in Human Ecological Adjustments, eds. Anthony Leeds and Andrew P. Vayda, Publication #78 (Washington, DC: American Association for the Advancement of Science, 1965), 43-65; Evadene Burris Swanson, "The Use and Conservation of Minnesota Game, 1850-1900," MA Thesis, University of Minnesota, 1940.

3 Several general studies of the allotment/forced assimilation policy are: Frederick E. Hoxie, A Final Promise: The Campaign to Assimilate the Indians, 1880-1920 (Lincoln: University of Nebraska Press, 1984); Leonard Carlson, Indians, Bureaucrats and Land: The Dawes Act and the Decline of Indian Farming (Westport: Greenwood Press, 1981); D.S. Otis, The Dawes Act and the Allotment of Indian Lands, ed. Francis P. Prucha (Norman: University of Oklahoma Press, 1973); H. Craig Miner, The Corporation and the Indian: Tribal Sovereignty and Industrial Civilization in Indian Territory, 1865-1907 (Columbia: University of Missouri Press, 1976).

⁴ Indian Office Reports, 1867:397; 1868:301; 1870: 305; 1871: 588, 592; 1872: 210; 1874: 195; 1875: 53,298; 1876: 84; 1877: 129; 1878: 81; U.S. Statutes at Large 17: 189, 534; 18: pt. 3, 173-4.

⁵ The National Archives catalogues the annual censuses of reservation populations as "Bureau of Indian Affairs' censuses." However, the term "Indian Office" was used in the late 19th and early 20th centuries. In this essay, the label "BIA censuses" will be used to describe the censuses to avoid confusion about archival sources. However, the term "Indian Office" will be used to refer to the institution that later became the Bureau of Indian Affairs.

⁶ "Chippewa Indians in Minnesota," House Executive Documents 2747, #247, 51 Congress, 1 Session (1890) (aka "Report of the U.S. Chippewa Commission"); Bureau of Indian Affairs' Censuses, 1889-1907, National Archives and Records Service Microfilm Publications, Rolls #649-655; Records of the Chippewa Commission, Register of Arrivals, 1890-1899, BIA Record Group 75, Entry 1305, National Archives and Records Service. This document lists removals by band for each consecutive year. Furthermore, it records the timing of migrations by day, month and year, the chief of the band of origin, and the destination at White Earth of each household. As such, it represents the best source for statistics regarding migrations during these years. No other source, published or manuscript, records this detail in serial fashion.

The reporting of removal statistics by various representatives of the U.S. government was uneven at best. Irregular methods resulted in overlapping estimates, gaps in reporting and contradictory figures. Sources must be cross-checked to detect these variances and determine the most reliable figures.

⁷ Melissa L. Meyer, "Tradition and the Market: The Social Relations of the White Earth Anishinaabeg, 1889-1920," Ph.D. Dissertation, University of Minnesota, 1985; Patricia A Shifferd, "A Study in Economic Change: The Chippewa of Northern Wisconsin: 1854-1900," Western Canadian Journal of Anthropology 6 (1976): 16-41; Jeanne Kay, "Native Americans in the Fur Trade and Wildlife Depletion," Environmental Review 9 (1985): 118-130.

⁸ The historical literature dealing with community and society in the North American fur trade is vast and growing. Of most importance to the history of the White Earth Reservation are works that focus on the

genesis of mixed blood or metis people in the Great Lakes area. See the following: Jennifer S.H. Brown, Strangers in Blood: Fur Trade Company Families in Indian Country (Vancouver: University of British Columbia Press, 1980); Sylvia Van Kirk, "Many Tender Ties: Women in Fur Trade Society, 1670-1870" (Winnipeg: Watson and Dwyer Publishing Ltd., 1980); Jacqueline Peterson, "Prelude to Red River: A Social Portrait of the Great Lakes Metis," Ethnohistory 25 (1978): 41-67; Jacqueline Peterson, "Ethnogenesis: The Settlement and Growth of a 'New People' in the Great Lakes Region, 1702-1815," American Indian Culture and Research Journal 6 (1982): 23-64; Helen Hornbeck Tanner, "The Glaiize in 1792: A Composite Indian Community," Ethnohistory 25 (1978): 15-39; Olive P. Dickason, "From 'One Nation' in the Northeast to 'New Nation' in the Northwest: A Look at the Emergence of the Metis," American Indian Culture and Research Journal 6 (1982): 1-21.

9 The marked political factions at White Earth in the early 20th century stand in sharp contrast to the patterns Loretta Fowler analyzed in Arapaho Politics, 1851-1978: Symbols in Crises of Authority (Lincoln: University of Nebraska Press, 1982). See Melissa L. Meyer, "Tradition and the Market..."; Melissa L. Meyer, "Warehousers and Sharks: Chippewa Leadership and Political Factionalism on the White Earth Reservation, 1907-1920," Journal of the West 23 (1984): 32-46.

10 Several inspectors made reference to such cards located in the Agency Office as they did genealogical research to determine individuals' blood quantum. "Report in the Matter of the Investigation of the White Earth Reservation," House Reports 6336, #1336, 62 Congress, 3 Session (aka "The Graham Report"); "Report of E.B. Linnen and W.K. Moorehead," Sept. 30, 1909, BIA Record Group 75, Central Classified Files 150, 310, White Earth Agency, National Archives and Records Service; Abstracts of Testimony taken by Linnen and Moorehead can be found in "Records Relating to Investigation of Allotments - W.E. Agency," Entry 1260, Record Group 75, National Archives and Records Service; "Lists Showing the Degree of Indian Blood of Certain Persons Holding Land upon the White Earth Reservation in Minnesota and a List Showing the Date of Death of Certain Persons Who Held Land upon such Reservation," (Washington DC: Government Printing Office, 1911) (aka "The Hinton Roll" - available at the Minnesota Historical Society).

11 Records of the Chippewa Commission, Register of Arrivals, 1890-1899, BIA Record Group 75, Entry 1305, National Archives and Records Service; Melissa L. Meyer, "Tradition and the Market..."

12 Thomas Shearman conducted genealogical research as part of the effort to determine the blood quantum of individuals and, thus, resolve land fraud claims. "Report of Thomas G. Shearman, Asst. Attorney, Visit to and Conference with certain full-blood Indians, ordered by the Dept., April 15, in connection with the White Earth Controversy," May 6, 1913, BIA Record Group 75, Central Classified Files 150, National Archives and Records Service. Serendipitous documents such as this have been important in illuminating social relations at White Earth.

13 Many sources support this discussion of ethnic divisions among the White Earth Chippewa. Reservation residents and outside observers alike noted these differences. Agents' reports, congressional testimony, missionaries' accounts, newspapers, maps and oral tradition all reinforce this interpretation. For specific references see: Melissa L. Meyer, "Tradition and the Market....," chapters 4 and 7.

14 This discussion of Chippewa understandings of the terms "mixed blood" and "full blood" draws heavily on testimony taken by Ransom J. Powell in connection with his work on the 1920 Blood Roll. His records are located in the Ransom J. Powell Papers at the Minnesota Historical Society Archives.

15 Age pyramids detailing the relative distribution of Chippewa names and English or French surnames further reveal band differences. These measures are more accurate reflections of individual behavior. In this case, actual population totals are less important than the marked differences between bands in their choice of names. Any problems with the censuses would apply equally to all bands.

Names listed on the censuses do not necessarily reflect individuals' choices, per se. Chippewa names could change during an individual's lifetime. Initially, children could be named after a peculiar incident at their birth or some special power possessed by another Indian. Names could be added or completely changed as they grew. See Frances Densmore, "Chippewa Customs," Bureau of American Ethnology Publications 86 (1929): 52-3. Furthermore, the anglicization process was not uniform: some translated their Indian names into English and added an English first name; some took their gens name as a family name; nicknames stuck; lumber company employees and school officials assigned names arbitrarily; and names were misspelled and mistranslated phonetically. See Robert Ritzenthaler, "Acquisition of Surnames by the Chippewa Indians," American Anthropologist 47 (1945): 175-7. In addition, names recorded in the census often describe the individual with a Chippewa word, "old woman" or "little girl," rather

than providing actual names. Individuals might possess both an English name and a Chippewa name which could be used interchangeably. Many factors combined to create the listing of names contained in the BIA censuses. Even so, the patterning of surnames complements other social and demographic patterns that differentiated bands at White Earth.

16 Sr. Bernard Coleman, Where the Water Stops: Fond du Lac Reservation (Superior, Wisc.: Arrowhead Printing Co., 1967); Harold Hickerson, "The Genesis of a Trading Post Band: The Pembina Chippewa," Ethnohistory 3 (1956): 289-345; Alexander Ross, The Red River Settlement: Its Rise, Progress, and Present State, With Some Account of the Native Races and Its General History, to the Present Day (London: 1856); Jacqueline Peterson, "Ethnogenesis..."; Jacqueline Peterson, "Prelude to Red River..."; Jacqueline Peterson, "The People in Between: Indian-White Marriage and the Genesis of a Metis Society and Culture in the Great Lakes Region, 1680-1830," Ph.D. Dissertation, University of Illinois, Chicago Circle, 1981; Willoughby Babcock, "With Ramsey to Pembina: A Treaty-Making Trip in 1851," Minnesota History 38 (1962); Ella Hawkinson, "The Old Crossing Chippewa Treaty and Its Sequel," Minnesota History 15 (1934): 282-300; Rhoda Gilman, Carolyn Gilman and Deborah M. Stultz, The Red River Trails: Oxcart Routes between St. Paul and the Selkirk Settlement, 1820-1870 (St. Paul: Minnesota Historical Society Press, 1979); Sr. Bernard Coleman, Sr. Verona La Bud and John Humphrey, Old Crow Wing: History of a Village (Duluth: College of St. Scholastica, 1967); Edmond Danziger, The Chippewas of Lake Superior (Norman: University of Oklahoma, 1979).

17 Studies of interior Minnesota reservation are more limited. See: Harold Hickerson, The Chippewa and Their Neighbors: A Study in Ethnohistory (New York: Irvington Publishers, 1970); Harold Hickerson, "The Southwestern Chippewa: An Ethnohistorical Study," American Anthropological Society Memoirs #92 64 (1962); William Warren, History of the Ojibway Nation (Minneapolis: Ross and Haines, Inc., 1974); Roger and Priscilla Buffalohead, Against the Tide of American History: The Story of the Mille Lacs Anishinabe (Cass Lake: The Minnesota Chippewa Tribe, 1985); Government investigators conducted genealogical inquiries into the backgrounds of the 86 individuals accused by Charles Wright and others of having no rights at White Earth. Their efforts revealed very few marriages between these individuals with Lake Superior band origins and Mississippi band members. See: "Report of Thomas G. Shearman..."

18 BIA "censuses" reveal that surnames of Chippewa origins increased within particular bands after 1910. This timing and the differential patterning of surnames

supports the notion that traditional Indians retained Chippewa names longer and more often chose to anglicize them instead of adopting new English or French names.

19 A comparison of age pyramids showing surname distributions taken at 10 year intervals over the 30 year period between 1900 and 1920 reveals this pattern. The White Earth Mississippi band and the Leech Lake Pillager band illustrate the patterns evident at either end of the spectrum of band differences.

20 Less important for my study, but pertinent for broader comparative purposes is the theory of the demographic transition. Demographers and social historians of human societies all over the world have noted the phenomenon of decreasing mortality and subsequent fertility control leading to smaller household sizes as "modernization" occurs. However, at White Earth, statistics about average household size show substantially larger families among more market-oriented bands and smaller households among more traditional bands. This apparent reversal of anticipated patterns is difficult to explain. Do the large households among more market-oriented bands represent an early stage of the demographic transition where mortality has been reduced but fertility not yet restricted? Was epidemic disease and reduced fecundity responsible for smaller households among traditional bands? Some evidence suggests that traditional Indians had smaller households because of cultural practices involving lengthy lactation and herbally-induced abortions early in pregnancy. Such practices call into question the assumption of unrestricted fertility among so-called "primitive" people and fundamental assumptions associated with the theory of the demographic transition.

21 United States Census Office, Twelfth Census of the United States Taken in the Year 1900, Manuscript form for White Earth Reservation, T623 White Earth Reservation, Roll # 748 Becker County, Roll # 756; United States Census Office, Thirteenth Census of the United States Taken in the Year 1910, Manuscript form for White Earth Reservation, T624 Becker County, Roll # 689, Mahanoy County, Roll # 710.

22 For an elaboration of the types of documentary evidence available, see: Melissa L. Meyer and Russell Thornton, "Indians and the Numbers Game: Quantitative Methods in American Indian History," In New Directions in American Indian History, ed. Colin Callaway (Norman: University of Oklahoma, forthcoming, 1987).

23 I want to emphasize that I will not be attempting family reconstitution or cohort analysis until evaluation

of the documents proves that such exercises will be possible. Massive land fraud, alienation of resources and subsequent out-migration may have created chaotic conditions on the Reservation. Such upheaval may prevent the tracing of a statistically-significant number of individuals through time. Before I embark on such an undertaking, I propose to link individuals through several documents at 2 points in time and compare the pertinent measures taken. Frederick Hoxie's paper will discuss the potential of cohort analysis and family reconstitution.

24 An earlier discussion of the utility of considering Chippewa life cycle in any analysis of family types at White Earth can be found in: Melissa L. Meyer, "The Historical Demography of White Earth Indian Reservation: The 1900 U.S. Federal Census Considered," The American Indian Culture and Research Journal 6 (1982): 29-62.

25 The 1920 Blood Roll was constructed through blatantly racist techniques to distinguish between mixed bloods and full bloods on the White Earth Reservation. Physical anthropologists conducted examinations of hair, teeth, and reactions of the skin to make their determinations. See, Melissa L. Meyer, "Tradition and the Market...", and David L. Beaulieu, "Curly Hair and Big Feet: Physical Anthropology and Land Allotment on the White Earth Chippewa Reservation," The American Indian Quarterly (1985).

Because of the racist methods used, information on blood status contained in this document does not interest me. However, included in the 1920 Blood Roll is a listing of each individual's allotments of land (often several) by township and range lines. Thus, it is a more complete compendium of allotments made than any single allotment roll for the purposes of comparing allotment patterns and residence patterns by band.

26 My predictions are informed by an understanding of demographic processes and literature concerning the urban migrations of American Indians. See especially: Elaine M. Neils, Reservation to City: Indian Migration and Federal Relocation (Chicago: University of Chicago, Department of Geography, Research Paper No. 131, 1971); Jack O. Waddell and O. Michael Watson, eds., The American Indian in Urban Society (New York: University Press of America, 1984); Arthur Margon, "Indians and Immigrants: A Comparison of Groups New to the City," Journal of Ethnic Studies (1977).

27 Robert F. Berkhofer, Jr., "Native Americans," In Ethnic Leadership in America, ed. John Higham (Baltimore: Johns Hopkins University Press, 1978): 128.

28 Immanuel Wallerstein, The Modern World System: Capitalist Agriculture and the Origins of the European World Economy in the Sixteenth Century (New York: Academic Press, 1974); Immanuel Wallerstein, The Modern World System II: Mercantilism and the Consolidation of the European World Economy, 1600-1700 (New York: Academic Press, 1980); Eric R. Wolf, Peasant Wars of the Twentieth Century (New York: Harper and Row, 1969); Richard White, The Roots of Dependency: Subsistence, Environment and Social Change among the Choctaw, Pawnee and Navajo (Lincoln: University of Nebraska Press, 1984); Joseph Jorgenson, "Indians and the Metropolis," in The American Indian in Urban Society, eds. Jack O. Waddell and O. Michael Watson (New York: University Press of America, 1984); 67-113; Joseph Jorgenson, "A Century of Political and Economic Effects on American Indian Society, 1880-1980," Journal of Ethnic Studies 6 (1978): 1-82; Gary C. Anders, "The Internal Colonization of Cherokee Native Americans," Development and Change 10 (1977): 41-55; Gary C. Anders, "The Reduction of a Self-Sufficient People to Poverty and Welfare Dependence: An Analysis of the Causes of Cherokee Indian Underdevelopment," American Journal of Economics and Sociology 40 (1981): 225-38; Gary C. Anders, "Theories of Underdevelopment and the American Indian," Journal of Economic Issues 39 (1980): 681-702; Cardell K. Jacobsen, "Internal Colonialism and Native Americans: Indian Labor in the United States from 1871 to World War II," Social Science Quarterly 65 (1984): 158-171; Rolf Knight, Indians at Work: An Informal History of Native Indian Labour in British Columbia, 1853-1930 (Vancouver: New Star, 1978).

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Thomas D. Hall, "Incorporation in the World System: Toward a Critique," American Sociological Review 51 (1986): 390-402; Duane Champagne, "Social Structure, Revitalization Movements and State Building: Social Change in Four Native American Societies," American Sociological Review 48 (1983): 754-763; Russell Thornton, "Nineteenth Century Cherokee History," American Sociological Review 50 (1985): 124-127; Duane Champagne, "Cherokee Social Movements: A Response to Thornton," American Sociological Review 50 (1985): 127-130; Duane Champagne, "Strategies and Conditions of Political and Cultural Survival in American Indian Societies," Occasional Paper #21, Cultural Survival Inc. (1985); Duane Champagne, "Culture, Differentiation and Environment: Social Change in Shingít Society," In Jeffrey Alexander and Paul Colomy eds., Differentiation and Social Change: Theory and Case Studies. New York: Cambridge University Press, forthcoming.

Comments on Melissa L. Meyer, "Censuses and American Indian Families: The
Case of White Earth Reservation, Minnesota, 1890-1920"
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February 26, 1987

Although there are tremendous differences between the cultures and settings for Melissa Meyer's study of the White Earth Reservation and my own work on European family history, I was pleased to find a great deal of common ground. It is easy to appreciate the care with which she reconstructs the settlement of the reservation, and her suggestions for future research seem very well directed. But there are several methodological and theoretical issues raised in the paper which may benefit from suggestions from another field. I will try to suggest here how some of the techniques of family history can enrich the understanding of cultural divisions emerging within the population of the White Earth Reservation.

The paper does three things. First, it describes the White Earth Reservation in both physical and cultural terms. Professor Meyer analyzes the dynamics of the settlement process identifying both the sources of its population and their destinations on the reservation. Second, she offers an analysis of differences between two sub-groups that moved to the reservation. Those identified as "full blood" Chippewa, a term that was cultural as much as genetic, had remained more traditional in their cultural and economic patterns and had been relatively less affected by contact with European culture. Those identified as "mixed bloods" often had both economic contact and intermarriage with non-Indians. Their participation in the fur trade had made them more market oriented, and the term "mixed blood" referred to cultural patterns associated with the market. Third, the paper describes some of the sources available for studying the White Earth Reservation and their possible applications. In particular, Professor Meyer draws attention to the possible linkage of Bureau of Indian Affairs enrollment lists and federal censuses.

In this comment I prefer to focus on the last two parts of the paper. It is more in keeping with the spirit of both the paper and the conference to call attention to the possibilities for research with these sources. I would emphasize, however, that the descriptive section of the paper includes a very interesting and useful discussion of the ecological zones on the reservation. Professor Meyer does an excellent job of describing the interaction between previous cultural and economic patterns and the various ecological possibilities offered by the reservation during the process of settlement, and this is clearly an important area for future research.

Underlying much of the paper is the question: Does quantitative data support the conclusion of other historical sources that participation in the fur trade had created two ethnic and cultural groups within the reservation? This issue has important implications, because later political conflicts seem to have developed along ethnic lines. The answer is "yes," and Professor Meyer provides two examples of quantifiable differences between ethnic groups. The effect of European contact is apparent in naming practices. Certain bands within the reservation were more likely to use anglicized names than others. A transition from

Anishinaabe to anglicized names is apparent in all bands, but some bands made the transition earlier and more completely. A second ethnic contrast emerges in an analysis of household size. The market oriented bands were characterized by higher average family sizes than the more traditional bands.

Any mention of differences in family sizes is tantalizing to a historical demographer like myself. Other discussions at the conference lead me to believe that the higher rates of reproduction among "mixed bloods" at the beginning of this century are an important subject for study. Future research will need to examine alternative explanations for these differences. Sources described in this paper should make it possible to distinguish between the effects of mortality and fertility on family size and even to examine evidence of voluntary family limitation.

In this connection, Professor Meyer mentions in a footnote the debate over the prevalence of fertility control in pre-modern populations. Her discussion leaves the incorrect impression that all demographers believe that no practices affecting fertility were known by primitive people. Many, perhaps most, demographers believe that virtually all populations follow some traditional practices tending to reduce fertility. Following Louis Henry, however, it is common to distinguish between the conscious intention to limit family size and practices that have a different goal. For example, extended lactation and post-partum abstinence can have significant effects in lowering fertility. But research from Africa has found that these practices are intended to safeguard the health of the infant and mother by people who otherwise value high fertility. It will be extremely interesting to learn whether voluntary family limitation was practiced in traditional Indian society and how contact with European populations changed these practices. The study of family patterns among American Indians is certain to be of great interest to demographers as well as historians.

In addition to the demographic questions raised by these data, the sources described by Professor Meyer address a broad range of questions about family dynamics. She points out that her evidence showing the predominance of nuclear families does not preclude more complex family types at different stages of the family life cycle. Even families following a complex developmental cycle often pass through a phase in which the household contains only a nuclear family. I would add that demographic factors (high mortality, migration, and high rates of population increase) can increase the proportion of nuclear families by increasing the ratio of nuclear to non-nuclear kin in the population as a whole. The sources available for the White Earth Reservation should allow studies that will identify the dynamics of family life cycles. This research may reveal new information about the process of cultural change that divided the people of the White Earth Reservation.

In order to examine some of these possibilities I turn to a discussion of the sources described in the paper: the federal censuses and the Bureau of Indian Affairs enrollment lists. The manuscript censuses of 1900 and 1910 are now available on microfilm, and Professor Meyer has elsewhere described them and some of their potential uses. These censuses are particularly useful for studying the family, because they are the earliest available censuses that included retrospective questions about

children ever born to each woman. Recent national samples from these censuses have stimulated a great deal of demographic research, and methods have been developed for deriving estimates of fertility and mortality from these retrospective questions.

The suggestion that these records would lend themselves to a family life cycle analysis is a good one, but an additional concept widely used in family history may also be productive. Historians of the European family frequently use the "family economy" as an organizing concept. This idea draws attention to the economic interdependence of family members and helps to integrate the analysis of economic choices with the study of family composition and life cycle.

A family economy analysis may be particularly fruitful if linked to the economic differences across the ecological zones of the reservation. We may expect differences in family patterns between bands that chose to engage in market oriented activities and those following a more traditional way life. One may ask how engagement in the market affected the family life cycle and whether this may be linked to shifting patterns of authority within the family. The paper offers a hypothesis linking allotments and sales after 1907 to out-migration and the decline of geographically based communities, which is a good example of this kind of reasoning.

The Bureau of Indian Affairs enrollment lists may hold the greatest potential for future analysis, but they also pose more complex problems. There is an overall problem of evaluation of their coverage and reliability. Who was covered? How were listings revised when individuals changed households or moved? How accurate and timely were revisions to reflect births and deaths? In the absence of documentation about the procedures used to compile these lists, they must be examined carefully. These lists are called censuses, but Professor Meyer points out that they do not seem to have involved actual surveys on the part of Bureau of Indian Affairs officials. Instead, they were apparently copied from cards kept in the Agency Office. Even though the coverage of these documents may not be ideal, they may be highly accurate. If benefits, such as annuities or land allotments, were tied to enrollment in these lists, financial incentives may have encouraged accurate record keeping.

A variety of internal and external tests can be devised to evaluate these lists. For example, one might examine how long it takes for these lists to reflect births and deaths of individuals for whom these dates are known. If information was entered promptly, there is more reason to believe in the accuracy of these lists. Professor Meyer proposes to link the lists to the federal censuses, which will provide the most comprehensive test of their quality. One should not, however, expect to find perfect agreement between the enrollment lists and the census. Populations are constantly in flux, and some disagreement between the lists and the census must be anticipated. The task is to identify the strengths and weaknesses of the enrollment lists. It is less important to compare them to an arbitrary and absolute standard of accuracy, than to discover what questions they can and cannot answer.

As an example of this process of evaluation we may point to one of the problems that Professor Meyer encountered in using the Bureau of

Indian Affairs enrollment lists: an unreasonably high proportion of individuals listed in single person households. The number of solitaires was so large, that Professor Meyer judged it appropriate to exclude all single person households from her estimates of average family sizes. This is a troubling, if unavoidable, procedure, because some "true solitaires" were undoubtedly excluded along with the spurious single person households.

One is inclined to suspect that there is an administrative reason for the high proportion of solitaires. For example, when a marriage involved spouses from two different bands, the agents may have continued to list them with their respective bands while listing the offspring in only one place. This type of recording practice would create a number of apparent solitaires and single parent households. While this procedure would make it more difficult to use the enrollment lists as indicators of household composition, other possibilities present themselves. The proportion of solitaires may be a rough index of the amount of intermarriage between bands. Furthermore, the implicit rules followed by the agents in assigning individuals to bands may reveal to us their assumptions about band membership in ambiguous cases like intermarriage. Obviously, this explanation of the prevalence of solitaires is purely speculative, but the issue can be resolved by linking the enrollment lists and the federal census.

The problem of solitaires underlines the difference between the Bureau of Indian Affairs enrollment lists and a true census, but a census is not necessarily the more informative document. A census gives only a "snapshot" describing a population at a moment in time. Records like the enrollment lists often derive from systems of continuous surveillance which make them continuous in time. Over time we see each individual change, not only aging but moving among statuses and households. Instead of a snapshot with one moment frozen in time, we have a moving picture in which changes have been recorded. Longitudinal data like this potentially allow us to ask more complex questions than we can with cross-sectional data. In the best circumstances we can link individual actions and decisions to the demographic characteristics of individuals and their families.

In most research in demography and family history, for example, there is a tendency to separate questions of demographic dynamics (fertility, mortality, marriage, and migration) from questions about family composition. This derives largely from the separation of these questions in the sources. The censuses, from which we derive measures of family composition, tell us little about dynamics, and the various types of continuous registration from which we derive vital rates rarely include evidence about family composition. Longitudinal data of the sort found in the enrollment lists allows us to put these two types of analysis together. We can ask how family composition affected such behaviors as marriage, migration, employment and schooling. Were adult children with living parents more or less likely to intermarry? Were sons in large families more or less likely to out-migrate? Were children in small families more or less likely to go to school?

This type of analysis also raises questions which are often ignored by the family cycle approach. In particular, the family cycle tends to

view the family from the point of view of the (male) head of household and neglects possible conflicts within households. Conflicts between the interests of individuals and their families are common, and the resolutions of these conflicts bring us closer to the ways in which decisions are actually made. When did children sacrifice their own economic or social interests to those of their parents, siblings, or other kin? Questions of this sort can be expanded from the immediate family to larger kin or social groups.

In this comment I have tried to suggest ways in which the quantitative sources available for the White Earth Reservation can be used to go beyond confirming inferences drawn from other types of documents. It seems to me that the censuses and Bureau of Indian Affairs enrollment lists can lead to new ways of viewing the differences in family and economic behavior between the traditional and market oriented populations. They can also link together the analysis of the economic environment and the cultural priorities of these two emerging groups. As Professor Meyer points out: "Mindless manipulation of data and statistics produces only meaningless measures." All historical work is more productive when it is motivated by a clear conception of issues, and quantitative research requires more attention to the formulation of questions than other historical methods.

Tribal History, Tribal Population, and Tribal Membership Requirements:
The Cases of the Eastern Band of Cherokee Indians,
the Cherokee Nation of Oklahoma,
and the United Keetoowah Band of Cherokee Indians in Oklahoma

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Tribal History, Tribal Population, and Tribal Membership Requirements

In July, 1986, the U.S. Bureau of Indian Affairs listed 306 American Indian tribes with which it had relations (including one American Indian tribe in Alaska and two unorganized tribes) (U.S. Bureau of Indian Affairs, 1986a). Most of these tribes were organized under the Indian Reorganization Act of 1934, subsequent decisions by the Indian Claims Commission, and/or state authority. Most also operated on the basis of written constitutions (and bylaws and corporation charters), typically containing specific criteria for formal membership in the tribe. Membership requirements may involve blood quantum, lineage, enrollment or allotment status, and residence, often in some complex combination. Considerable diversity from tribe to tribe is shown in virtually all membership requirements. Blood quantum is probably as diverse as any, however. Many tribes do not require a minimum blood quantum; when one is specified the modal requirement is one-fourth, although some tribes require as much as one-half and several tribes--particularly in California and Oklahoma--require a minimum of one-eighth or one-sixteenth. Minimum required blood quantum for these 306 tribes are shown in the Appendix, along with population sizes for these tribes: they totaled some 900,000 members. This indicates that only some two-thirds of the 1,366,676 American Indians counted in the 1980 U.S. census (Swagerty and Thornton, 1982:92; U.S. Bureau of the Census, 1984:2, table 4)¹ were actual members of federally recognized American Indian tribes.

Table 1 shows the population sizes of these American Indian tribes grouped

[Table 1 about here]

by specified required blood quantum. As shown in the table, twenty-one tribes had minimum blood quantum requirements ranging from one-third to one-half; 163 tribes had requirements ranging from one-sixteenth to one-fourth; and fifty-four

tribes specified no minimum blood quantum requirements. (Not shown in the table are the sixty-eight tribes for which a blood quantum requirement is not specified [or not known].) Average tribal size of the groupings ranged from 2,196 for tribes with one-third or more required blood quantum to 3,352 for tribes with one-sixteenth to one-fourth required blood quantum to 5,344 for tribes with no specified required blood quantum. The median sizes were 546, 788, and 1,221, respectively. As one might expect, the lower the required blood quantum, the larger the tribe.

Enrolled Cherokee and Cherokee in the 1980 Census

Today there are three federally recognized Cherokee tribes: the Eastern Band of Cherokee Indians; the Cherokee Nation of Oklahoma; and the United Keetoowah Band of Cherokee Indians in Oklahoma. As shown in table 2, they had a combined enrollment of 78,881 in 1981. Shown in table 3, however, the 1980

[Tables 2 and 3 about here]

census counted 232,080 self-reported Cherokee. Thus probably only about one-third of the Cherokee counted in the 1980 census were actually enrolled in one of the three Cherokee tribes. This compares with the above two-thirds figure of all American Indians in the 1980 census. Cherokee in the census were, therefore, about one-half as likely to be enrolled as were American Indians generally.

The 1980 census also collected information as to those Cherokee living on the Eastern Cherokee Reservation in North Carolina and in the "historic Cherokee area" of Oklahoma. Shown in table 4, only about 4,600 Cherokee lived on the

[Table 4 about here]

Eastern Cherokee Reservation in 1980; almost all of whom were enrolled, whereas 8,381 individuals were enrolled in the Eastern Band of Cherokee Indians at about this time. In Table 5 is presented similar information for the "historic

[Table 5 about here]

Cherokee area" of Oklahoma.² These data show, in contrast to the Eastern Band of Cherokee, that while 38,243 Cherokee lived in this historic area, only about 50 percent of them were actually enrolled, and that only about 25 percent of the then total enrollment of the Cherokee Nation of Oklahoma (63,000) and the United Keetoowah Band of Cherokee Indians in Oklahoma (7,500) were actually living on former Cherokee lands.

Cherokee Population History: 1900-1980

The Cherokee removal from the Southeast into Indian Territory during the 1830s entailed tremendous population loss and put considerable strain on the tribal organization. The tribe was split into two geographic entities: a small group remained in the Southeast, while a much larger group joined already established early immigrants in Indian Territory. Political divisions stemmed from the geographic separation, and internal divisions arose from the forfeiture of tribal lands.

Nevertheless, both the Cherokee remaining in the Southeast and the Cherokee in Indian Territory were able to reestablish tribal life. Those in the Southeast became known as the Eastern Band of Cherokee in North Carolina, and many reside in that state on the Eastern Cherokee Reservation. The Cherokee in Indian Territory established a new tribal capital at Tahlequah and a new tribal constitution, and became known as the Cherokee Nation. As a tribe, the Cherokee Nation prospered in Indian Territory, even "adopting" some Delaware and Shawnee.

In 1887 the Dawes Act was approved by the U.S. Congress, however. It provided for the possible allotment of American Indian tribal lands in severalty, that is, to individual tribal members. It did not extend to the Cherokee Nation, to the other so-called Five Civilized Tribes, or to certain other American Indian tribes (see Prucha, 1973:171-74). The so-called Dawes Commission was created to negotiate with the Cherokee Nation and the other Five Tribes "for the purpose of the extinguishment of the national or tribal title to

any lands within that Territory now held by any and all of such nations or tribes" (Prucha, 1973:189). A commission to the Five Civilized Tribes was created; subsequently an agreement was made in 1900 with the Cherokee Nation for the allotment of tribal lands (see the Institute for the Development of American Indian Law, n.d.:77-88). In 1902 the Act to Allot the Lands of the Cherokee Nation was formally approved (The Institute for the Development of Americann Indian Law, n.d.:89-98). A final tribal roll was to be established for the purpose of this allotment; the tribal government of the Cherokee was not to continue past March 4, 1906. The Cherokee population in Indian Territory at this time was about 35,000 (U.S. Department of the Interior, 1903:172). The total population residing in the Cherokee Nation in 1900 was, however, 101,754 (U.S. Bureau of the Census, 1907:8, table 2); the difference, of course, was primarily composed of whites and blacks.

As one might expect, the situation resulted in considerable turmoil in the Cherokee Nation. Many full bloods resisted, not wanting to allot the lands, particularly to mixed-bloods and intermarried whites, and they avoided enrollment for a time. During this time the earlier-founded Keetoowah Society became factionalized; one leader opposed to allotment, Redbird Smith, withdrew, taking his followers with him. Then, "[t]hese ultra-conservative Keetoowahs formed their own organization which would become known as the Nighthawk Keetoowahs" (Hendrix, 1983b:80). Redbird Smith became the chairman. Eventually, however, Cherokee lands were allotted to individual tribal members, as established by a tribal roll, and as pursuant to the act of 1902 and subsequent acts of Congress.

On July 1, 1907, the final and complete roll of the Cherokee Nation was closed. It contained the names of 41,798 citizens: 31,400 Cherokees by blood, including 8,698 full bloods; 197 registered Delaware-Cherokee; 4,991 Cherokee minors; 286 intermarried whites; 4,305 freedmen; and 619 freedmen minors (U.S. Department of the Interior, 1907:292; also Wardell, 1977 [1938]:332-33). That

year the Cherokee Nation and Indian Territory became part of the then created state of Oklahoma. At this time, given the large influx of whites and others into the Cherokee Nation, the total population in the area that was the Cherokee Nation was 140,415 (U.S. Bureau of the Census, 1907:8, table 2).

The Cherokees in 1910

The 1910 U.S. census enumerated 265,683 American Indians (plus 25,331 Native Americans in Alaska): 31,489 were Cherokee; they were the largest American Indian group in the United States (U.S. Bureau of the Census, 1915:10, table 1; 14; 15, table 8). The Cherokee were enumerated in twenty-five states, but only Oklahoma and North Carolina had more than 1,000: Oklahoma had 29,610; North Carolina had 1,406 (U.S. Bureau of the Census, 1915:17, table 9).

Full Bloods and Mixed-Bloods

The census was not an enumeration of Cherokee "citizens;" rather, it was of Cherokee by "blood." The 1910 census (1915:10) report pointed out, however: "[t]hat all persons of mixed white and Indian blood who have an appreciable amount of Indian blood are counted as Indians, even though the population of white blood may exceed that of Indian blood." Only 6,900, or 21.9 percent, of the Cherokees were enumerated as full blood (U.S. Bureau of the Census, 1915:33, table 14). (The blood quantum of 260 Cherokee, or .8 percent, was not reported.) This compared with 56.5 percent of the total American Indian population enumerated as full blood (U.S. Bureau of the Census, 1915:31, table 12). (Blood quantum of 8.4 percent was not reported.) Percentage of Cherokee full bloods varied greatly from Oklahoma to North Carolina: 5,919 or 20.0 percent of Oklahoma Cherokee were reported as full blood; 934 or 65.4 percent of North Carolina Cherokee were reported as full blood (see table 6). Of those

[Table 6 about here]

Cherokee enumerated as full-blood American Indians, the overwhelming majority

were enumerated as full-blood Cherokee, as shown in table 7. This was greater than the 92.8 percent of the full-tribal blood in the total U.S. American Indian population.

[Table 7 about here]

Of the 24,329 Cherokee enumerated as mixed-blood, by far the most were of solely white and American Indian mixture: 96.6 percent. This was somewhat greater than for the total U.S. American Indian population: 94.2 percent. A significant percentage of North Carolina Cherokee reported a triracial black-white-American Indian mixture, however (see table 8). With the

[Table 8 about here]

notable exception of the North Carolina Cherokee, most of the solely white-mixed Cherokee were over 50 percent of white blood, as shown in table 9. Less than

[Table 9 about here]

half of the solely white-mixed North Carolina Cherokee were over 50 percent white. About 50 percent of the white-mixed-blood American Indians in the total U.S. population were over 50 percent white. As shown in table 10, most of the

[Table 10 about here]

solely black mixed Cherokee were over 50 percent black, although the percentage is less dramatic than is the case with the solely white-mixed Cherokee: overall, 63.5 percent as compared with 82.5 percent. This also is far greater than the case with the total American Indian population of the United States: only 34.6 percent of solely black-mixed American Indians were over 50 percent black.

Sterility, Fertility, Fecundity

The U.S. Bureau of the Census also reported 1910 data on the sterility (inability to have children), fecundity (potential to have children), and fertility (actual child-bearing) of American Indian women "who were between 15 and 44 years of age, who had been married for at least one year, and who were neither widowed nor divorced nor married a second or subsequent time" (U.S.

Bureau of the Census, 1915:157).³ The data show a significant, general pattern of lower reproductive behavior of full-blood marriages than for other types of marriages. For example: 10.4 percent of marriages among full bloods (of the same tribe) had born no children, as compared with only 6.9 percent of marriages of mixed-bloods (white only); the average number of children born of these full-blood marriages was 4.5 but an average of 5.3 were born of mixed-blood marriages; and, finally, these full-blood marriages had 70.0 of their children surviving whereas the mixed-blood marriages had 77.8 of their children surviving (see table 11).

[Table 11 about here]

The Bureau of the Census concluded in this regard:

the increase of the mixed-blood Indians is much greater than that of the full-blood Indians, and that unless the tendencies now at work undergo a decided change the full-bloods are destined to form a decreasing proportion of the total Indian population and ultimately disappear altogether. (U.S. Bureau of the Census, 1915:159)

Along these lines, some few data were reported for specific American Indian tribes, including the Cherokee in Oklahoma (see table 11, again). Some 7.3 percent of Oklahoma Cherokee full-blood marriages had no children, but only 6.4 percent of Oklahoma Cherokee mixed-blood marriages had no children; the average number of children in Cherokee full-blood marriages was 3.7, somewhat higher than the average of 3.5 children in mixed-blood marriages; and Cherokee full-blood marriages had 71.8 percent of their children surviving whereas 82.0 percent of the children were surviving in mixed-blood marriages. For the Cherokee this suggests, too, that the full-blood population was decreasing in relative proportion to the mixed-blood population simply because of differences in reproductive behavior and outcome.

Of course, the mentioned changes in the proportions of full bloods and mixed-bloods in the Cherokee population, as well as in the total American Indian

population, were due to other factors as well as to reproductive behavior, e.g., mortality differences, marriage patterns. No data were available on mortality differences; however, the percent of surviving children does get at this issue. Similarly, marriages were not addressed either. That the change was occurring can also be seen in the data presented in table 12, reporting percentage of the

[Table 12 about here]

populations under twenty years of age. As seen in the table, only 44.7 percent of the total American Indian full blood population was under twenty, whereas 62.7 percent of the mixed-blood population was under twenty. The data for Cherokees show a similar pattern. This suggests--in fact, indicates--that more of the children being born and surviving were of mixed- rather than full blood.

The Cherokee Population in 1930

By 1930, the total American Indian population had grown to 332,397, plus 29,983 Native Americans in Alaska, as counted in the U.S. census (U.S. Bureau of the Census, 1937:2, table 1). As enumerated, the Cherokee population numbered 45,238: 40,904 in Oklahoma; 1,963 in North Carolina; the remainder in forty-two other states, particularly Alabama, Virginia, and California (U.S. Bureau of the Census, 1937:59-60, table 9). The Cherokees were still the largest American Indian people in the United States; they were also "geographically by far the most widely distributed" (U.S. Bureau of the Census, 1937:43).

Enumeration procedures in this census differed slightly from those in 1910, viz:

The enumerators in the Fifteenth Census were instructed to return as Indians, not only those of full Indian blood, but also those of mixed white and Indian blood, 'except where the percentage of Indian blood is very small,' or where the individual was 'regarded as a white person in the community where he lives.' . . . 'A person of mixed Indian and Negro blood should be returned as a Negro unless the Indian blood predominates

and the status as an Indian is generally accepted in the community.'
(U.S. Bureau of the Census, 1937:1)⁴

Full Bloods and Mixed-Bloods

As enumerated, there were large changes from 1910 to 1930 in the relative proportion of full and mixed-bloods in the total American Indian populations, as can be seen from comparing table 6 above with table 13: The American Indian

[Table 13 about here]

population changed from over half full bloods to less than half full bloods during these two decades. The change for the total Cherokee was far less dramatic, as shown in the tables; however, the Cherokee population was already predominantly mixed-blood by 1910. The North Carolina Cherokee population did change greatly in this regard, however: from 66.4 percent full bloods in 1910 to 38.7 percent full bloods in 1930. Also, in 1910 the North Carolina Cherokees had a higher proportion of full bloods than the total American Indian population, but in 1930 they had a lower such proportion.

As in 1910, the mixed-blood segment of the American Indian population in 1930 was younger than the full blood segment: 62.7 percent were under twenty years of age as opposed to 44.7 percent of the full blood segment (see table 14). However, there was a slight increase in the proportion of full bloods

[Table 14 about here]

under twenty years of age from 1910 to 1930, but a slight decrease in this regard for the mixed-blood segment (see tables 12 and 14, again). Regarding only the Cherokee, the proportion of full bloods under twenty years of age decreased slightly; and the proportion of mixed-bloods under twenty years of age also decreased, but the decrease was of a larger magnitude than for the full bloods. (No separate data were available for Oklahoma and North Carolina Cherokee in 1930.) Some of these differences are surely attributable to enumeration procedures and errors in the census; some surely reflect real

changes in the populations over time.⁵

Cherokees Elsewhere, 1910 to 1930

During the decades from 1910 to 1930 the Cherokee became an even more geographically dispersed population. In 1910, Cherokee were living in only twenty-three states other than Oklahoma and North Carolina, as shown in table 15. In 1930, however, Cherokee were living in over thirty-seven states other than [Table 15 about here]

Oklahoma and North Carolina (see table 15, again). More, and as also shown in table 15, no state other than Oklahoma or North Carolina had as many as 100 Cherokee in 1910 (and most had less than twenty). In 1930, however, eight states other than Oklahoma and North Carolina had more than 100 Cherokee (and twenty-two states had over twenty Cherokees).

The Cherokee Population at 1970

From 1930 to 1970, the Cherokee population, as enumerated in U.S. censuses, grew to total 66,150. As we see, however, different processes were operating in the different segments of the Cherokee population.

The Western Cherokee to 1970

By far, the largest amount of growth occurred in the western Cherokee population of Oklahoma and, now, significantly elsewhere in the United States. No census data are available for the separate decades between 1930 and 1970, but other figures are: One estimate placed the number of Oklahoma Cherokees of at least one-fourth blood quantum at more than 25,600 in about 1950 (Debo, 1951:5); another estimate placed the number of people in Oklahoma with "a claim to being Cherokee" at 75,000 by about the mid-1950s (Wahrhaftig, 1968:510)! As to the former estimate, one authority has asserted: "It is my considered judgment that practically all of these are half-bloods or over" (Debo, 1951:5). As to the

second estimate, another authority has pointed out that most of the 75,000 should be considered only "white Americans of Cherokee ancestry" (Wahrhaftig, 1968:510). This estimate was based on work conducted by anthropologist Sol Tax (1960) and his associates at the University of Chicago. The work was a map detailing the destruction of descendents of American Indians in self-identified American Indian "communities." Furthermore, it has been estimated by Albert L. Wahrhaftig that only about 9,500 Oklahoma Cherokee lived and functioned "in Cherokee settlements" in 1963. These were in five eastern Oklahoma counties formed from previous Indian lands: Adair (3,012); Cherokee (2,058); Delaware (1,838); Mayes (1,197); and Sequoyah (1,191) (Wahrhaftig, 1968:512, table 1).⁶ In addition, Wahrhaftig asserted that there were another 2,000 individuals who did not live in Cherokee settlements but functioned as a part of them (Wahrhaftig, 1968:513). Also, he asserted there were about 10,500 Oklahomans "[r]eared in Cherokee settlements and resident in Cherokee Nation" (Wahrhaftig, 1968:513), and 16,000 or more individuals "[r]eared in Cherokee settlements but not necessarily participants in Cherokee society, regardless of residence" (Wahrhaftig, 1968:513). He then argued that using the same criteria of Cherokee living in Cherokee settlements and functioning in traditional Cherokee institutions, there were only about 8,000 "conservative" Cherokee in 1902. In these terms, then, the "Cherokee" population increased by only a few thousand since then, and roughly three-fourths of the natural increase in the Oklahoma Cherokee population since 1902 had become assimilated into general American society (Wahrhaftig, 1968:518).⁷

The Eastern Cherokee to 1970

In the late 1950s, the population of The Eastern Band of Cherokee Indians living on the Qualla Boundary and the adjacent 3,200 Acre Tract was estimated at about 3,000, out of about 3,300 in the entire reservation area (Gulick, 1960:7). The work by Sol Tax (1960), mentioned above, noted 4,266 "Cherokee" in the area

in 1959. In 1960, the total population of the Eastern Band of Cherokee living in the area or not was 4,494 (Kupferer, 1968:89). It was also estimated that almost 75 percent of this population was one-half or over full blood (Gulick, 1960:17, table 1), even though membership could be extended to anyone at least one-thirty-second full blood (Gulick, 1960:16). (See table 16.)

[Table 16 about here]

Cherokees Elsewhere

Sol Tax and his associates also identified several other "Cherokee" communities in the 1950s: 104 "Cherokee" in central Pennsylvania, 820 "Cherokee" (Cajuns) in extreme southwestern Alabama, and 300 Cherokee in north-central California (Tax, 1960).

Cherokees East and West in the 1970s

By 1970, both Cherokee populations had grown; as mentioned, the 1970 census enumerated 66,150 Cherokees. However, not all of these were actual members of the respective Cherokee tribes, based on their membership requirements, as specified below. Only about 12,000 Cherokees were enrolled in The Cherokee Nation of Oklahoma in 1975 (Cherokee Advocate, 1986:15A). Some 8,381 individuals were enrolled in the Eastern Band of Cherokee Indians in 1974: 5,550 of these resided on tribal lands; 2,831 resided off tribal lands (Blankenship, 1978:6). However, the 1970 census enumerated only 3,455 residing on the Cherokee Reservation (U.S. Bureau of the Census, 1973:190, table 17). Also, a small number were enrolled in the United Keetoowah Band of Cherokee Indians in Oklahoma, as the Keetoowahs were now a formal American Indian tribe.

Membership Criteria

By the 1970s all three formally organized groups of Cherokees had established their own, unique membership criteria.

The Cherokee Nation of Oklahoma

Following Cherokee land allotment and the "dissolving" of the Cherokee Nation as a republic in 1906, the President of the United States was granted power by the U.S. Congress to appoint a "Principal Chief" of the Cherokee to conduct official business. During ensuing decades, the "Cherokee Nation" continued to exist, although it lay dormant in many respects. During the 1970s, however, there was a rebirth of the Cherokee Nation (as well as of many American Indian tribes throughout the United States). In large part, this was a result of a new policy by the federal government--under Presidents Lyndon B. Johnson and Richard M. Nixon--whereby American Indian rights to "self-determination" were reaffirmed. In 1971, the Cherokee Nation of Oklahoma elected its principal chief for the first time since 1902. On June 26, 1976, a new Cherokee constitution supplanting the still in effect Cherokee Nation Constitution of September 6, 1839, as amended, was approved.

The old constitution did not provide a specific set of qualifications for tribal membership. (Traditionally, tribal membership was determined by lineage: anyone born of a Cherokee mother was a Cherokee.) It did state, however:

The descendents of Cherokee men by all free women except the African race, whose parents may have been living together as man and wife, according to the customs and laws of this Nation, shall be entitled to all the rights and privileges of this Nation, as well as the posterity of Cherokee women by all free men. (The Constitution and Laws of the Cherokee Nation, 1839-51, 1852:57)

Subsequently, various acts were passed admitting certain Creek Indians (and their families) and certain white men to citizenship in the Cherokee Nation (see, for example, The Constitution and Laws of the Cherokee Nation, 1839-51, 1852:91-92, 173, 200, 205). Of course, the earlier discussed Agreement Between the Delaware and Cherokee Tribes in 1867 and Agreement Between the Shawnees and the Cherokees in 1869 admitted certain Delawares and Shawnees into the Cherokee

Nation. Additionally, the final roll established in 1907 also admitted numerous so-called freedmen and whites to "citizenship" in the Cherokee Nation. The new constitution of the Cherokee Nation provided a new, specific set of membership criteria. It simply stated in this regard:

All members of the Cherokee Nation must be citizens as proven by reference to the Dawes Commission Rolls, including the Delaware Cherokees of Article II of the Delaware Agreement dated the 8th day of May, 1867, and the Shawnee Cherokees as of Article III of the Shawnee Agreement dated the 9th day of June, 1869, and/or their descendants.

(Constitution of the Cherokee Nation, 1976:Section 1)

Thus, no minimum blood quantum is specified for membership, and descendants of all those appearing on the Dawes Commission Rolls--including blacks and whites--are eligible for membership in the Cherokee Nation by Oklahoma.

The Eastern Band of Cherokee Indians

Membership in The Eastern Band of Cherokee Indians is somewhat more complex. New Eastern Band of Cherokee Indians tribal rolls were established in 1924 and 1931. The so-called Baker Roll of 1924 was in response to the federal government assuming a "trust responsibility for the Band: it specified one-thirty-second degree of Indian blood as a minimum for membership. The roll of 1931 was basically a supplement to the Baker Roll: it added 31 individuals to the roll" (Blankenship, 1978:103). Rolls and membership criteria were subsequently revised. Membership today consists of those on earlier rolls in addition to those meeting certain requirements: those living on August 21, 1957 who applied for membership before August 14, 1963 must "possess at least 1/32 degree of Eastern Cherokee Indian blood, and those persons who apply for membership on or after August 14, 1963 [must] possess at least 1/16 degree of Eastern Cherokee Indian blood" (Blankenship, 1978:128).

United Keetoowah Band of Cherokee Indians in Oklahoma

On October 3, 1950, the Keetoowahs became an organized band of American Indians under the Indian Reorganization Act of June 18, 1934 and the Oklahoma Indian Welfare Act of June 26, 1936. Membership in the Band was specified as:

All persons whose names appear on the list of members identified by a resolution dated April 19, 1949, and certified by the Superintendent of the Five Civilized Tribes Agency on November 6, 1949; Provided, that within five (5) years after the approval of this Constitution and Bylaws, such roll may be corrected by the Council of the United Keetoowah Band of Cherokees, subject to the approval of the Secretary of the Interior. (Constitution and By-laws of the United Keetoowah Band of Cherokee Indians, Oklahoma, 1950: Section 1)

In addition, it was specified: "The governing body of the Band shall have power to prescribe rules and regulations giving future membership" (Constitution and By-laws of the United Keetoowah Band of Cherokee Indians, Oklahoma, 1950: Section 2).

Conclusion

In the 1970s Ronald Trosper examined tribal membership and definition on the Flathead Indian Reservation in northeastern Montana, where he sought to answer two basic questions: "(1) Why did the Confederated Salish and Kootenai Tribes of the Flathead Indian Reservation exist as a legally defined group in 1970? (2) Why did the Confederated Salish and Kootenai Tribes have 5,500 enrolled tribal members in 1970?" (Trosper, 1976:256).

The first question was important, Trosper argued, because the tribes had been disappearing as recently as fifty years ago, and in 1954 a bill had been prepared in the U.S. Congress to terminate the tribes, abolish the reservation, and end the Confederated Salish and Kootenai's status as a federally recognized tribe. The second question was important, according to Trosper, because the

tribes were composed mainly of mixed-bloods:

Thirty percent have less than one-fourth Indian blood, 40% are between one-fourth and one-half, and the remaining 30% have more than one-half Indian blood. Only 3% of the tribal membership is full-blood. . . . The requirements to enroll children have tightened since 1935. If all those with Indian blood had been included, in 1970 the tribal population would have been 7,100 rather than the enrolled 5,500. The rule since 1960 has been that only those born with a quarter or more Indian blood can become tribal members. (Trosper, 1976:256)

To answer both questions, Trosper studied the inhabitants of the Flathead Reservation from 1860 to 1970. He (1976:271) concluded: "Tribal leaders adopted the policy of entrenchment in order to protect the existence of the reservation. This policy involved adopting a definition of Indian which led to a membership of 5,500." The new definition of Indian for tribal membership emphasized blood quantum rather than community, which had been stressed previously. Thus the Confederated Salish and Kootenai Tribes became more exclusive in determining membership in order to protect the tribes as entities. If they had not done so, they probably would not have survived as tribes, according to Trosper.

In 1981 there were 6,210 individuals enrolled as formal members of the Confederated Salish and Kootenai Tribes (see appendix). (In 1980, however, only 2,760 people identified as Salish or Kootenai lived on the Flathead Reservation: 2,112 were enrolled in the tribes and 303 did not report their enrollment status [U.S. Bureau of the Census, 1985:19].)

In contrast to the survival of the Confederated Salish and Kootenai Tribes are the histories of the three Cherokee tribes today: the Eastern Band of Cherokee Indians, the Cherokee Nation of Oklahoma, and the United Keetoowah Band of Cherokee Indians of Oklahoma.

Whereas the Confederated Salish and Kootenai Tribes attained tribal survival through development of a new policy of exclusiveness, the Cherokee in

Oklahoma have more or less maintained a policy of inclusiveness. This inclusiveness has seemingly also operated to assure tribal survival. As the Cherokee in Oklahoma lost their land, they came in increasing contact with whites and blacks, mixed with them, and became a population of predominantly mixed-bloods. This large mixed-blood population enabled them to achieve "political power" as a tribe and to protect a "core" of more full-blood, more traditional Cherokee, as Wahrhaftig argued. Perhaps without a reservation land base the Cherokee in Oklahoma--both the Cherokee Nation of Oklahoma and the United Keetoowah Band of Cherokee Indians in Oklahoma--had to adopt an inclusive policy for tribal membership. Without one, perhaps mixed-blood Cherokee, and even full-blood Cherokee, would have become "lost" in the total Oklahoma population, and the tribe would have ceased to exist. Indeed, it almost did for many decades. In contrast, the Eastern Band of Cherokee Indians, with their reservation land base, have become more exclusive in recent decades, although they continue with a fairly generous policy for tribal membership. (Interesting in this regard is the far greater percentages of one-half or over blood quantum members in the late 1950s than in the mid-1920s. The data for the two time periods are not directly comparable, however.) Finally, it may be pointed out that many of the tribes listed in the Appendix having no minimum blood quantum requirements for membership are to be found in Oklahoma, a state with only one reservation, that of the Osage. And, too, the thirty-five tribes in Oklahoma with known blood quantum requirements differ greatly from the total of American Indian tribes in minimum blood quantum requirements required for membership: 51.4 percent of Oklahoma tribes require no minimum, compared with 22.7 percent nationally; 45.7 percent require between one-sixteenth and one-fourth, compared with 68.4 percent nationally; and only 2.9 percent require as much as one-third, compared with 8.8 percent nationally.

Footnotes

1. The 1980 census also counted 42,162 Eskimo and 14,205 Aleutian Islanders for a grand total of 1,423,043 Native Americans (Swagerty and Thornton, 1982:92; U.S. Bureau of the Census, 1984:2, table B).
2. This was comprised of Adair, Cherokee, Craig, Delaware, McIntosh, Mayes, Muskogee, Nowata, and Ottawa counties and portions (urbanized area was excluded) of Rogers, Sequoyah, Tulsa, Wagoner, and Washington counties (U.S. Bureau of the Census, 1985:99, table 13).
3. This enabled the women in the description to have the following characteristics: "(1) They were of child-bearing age; (2) had been married long enough to have children; and (3) were living in the married state at the time of the enumerations" (U.S. Bureau of the Census, 1915:157).
4. It was further noted:

The census of 1910 was probably more accurate than that of 1930 in the enumeration by tribe and blood of those Indians whose tribal organization had broken down, and who were living as a part of the white community, or scattered through mountain areas. In North Carolina, and also in many other areas, the proportion of Indians shown in the census of 1930 as of full blood is much too high. This is particularly true of those tribes in which there is a large Negro admixture. (U.S. Bureau of the Census, 1937:1)
5. Data were also reported as to median age: the median age of the total American Indian population was 19.6 years--22.2 for full bloods and 16.4 for mixed-bloods; the median age of the Cherokee population was 17.7 years--24.2 for full bloods and 16.5 for mixed-bloods (U.S. Bureau of the Census, 1937:89, table 18).
6. According to this same author, this was considerably less than the number identified as Cherokee in the 1960 census enumeration (Wahrhaftig, 1968:512,

table 3).

7. It has been estimated that only about 10,000 people could speak Cherokee by the early 1960s (Chafe, 1962:165).

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Table 1. Membership of Federally Recognized American Indian Tribes Circa 1981,
by Required Blood Quantum

<u>Required Blood Quantum</u>	<u>Enrolled Members</u>	<u>Number of Tribes^a</u>	<u>Average Size</u>	<u>Median Size</u>
1/2-1/3	46,117	21	2,196	546
1/4-1/16	546,348	163	3,352	788
NMR ^b	<u>288,591</u>	<u>54</u>	5,344	1,221
Total	881,056	238	3,702	941

^aTribes with requirement unknown (or unspecified) are excluded.

^bNo minimum requirement.

Source: Appendix

Table 2. Population of the Three Cherokee Tribes,
Circa 1981

<u>Tribe</u>	<u>Population</u>
Eastern Band of Cherokee	8,381
Cherokee Nation of Oklahoma	63,000?
United Keetoowah Band of Cherokee Indians in Oklahoma	<u>7,500?</u>
Total	78,881

Source: Appendix.

Table 3. Persons Identifying as "Cherokee" in the
1980 Census

<u>Cherokee Identification</u>	<u>Number</u>
Cherokee	230,792
Eastern Cherokee	1,013
Western Cherokee	78
Etowah Cherokee	43
United Keetoowah	55
Northern Cherokee	21
Tuscola	<u>78</u>
Total	232,080

Sources: U.S. Bureau of the Census (1981,
1985:B-17).

Table 4. American Indian Population of Eastern Cherokee Reservation in 1980, by Enrollment Status

<u>Enrollment Status</u>	Population					
	Cherokee		Other American Indians		Total	
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>
Enrolled	4,546	97.6	97	58.8	4,643	96.3
Not enrolled	84	1.8	45	27.3	129	2.6
Not reported	<u>27</u>	<u>0.6</u>	<u>23</u>	<u>13.9</u>	<u>50</u>	<u>1.0</u>
Total	4,657	100.0	165	100.0	4,822	99.9

Source: U.S. Bureau of the Census (1985:18, table 4).

Table 2. Population of the Three Cherokee Tribes,
Circa 1981

<u>Tribe</u>	<u>Population</u>
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Cherokee Nation of Oklahoma	63,000?
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Northern Cherokee	21
Tuscola	<u>78</u>
Total	232,080

Sources: U.S. Bureau of the Census (1981,
1985:B-17).

Table 5. American Indian Population^a of Historic Cherokee Area^b of Oklahoma in 1980, by Enrollment Status

<u>Enrollment Status</u>	<u>Population</u>	
	<u>N</u>	<u>%</u>
Enrolled	17,638	46.2
Not enrolled	18,680	48.8
Not reported	<u>1,925</u>	<u>5.0</u>
Total	38,243	100.0

^aReported as American Indian, but presumably only Cherokee.

^bExcludes urbanized areas.

Source: U.S. Bureau of the Census (1985:99, table 13).

Table 6. American Indian and United States, Oklahoma, and North Carolina
Cherokee Population by Blood, 1910

	<u>Full Blood</u>		<u>Mixed-Blood</u>		<u>Blood Not Reported</u>		<u>Total</u>
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	
<u>American Indian</u>	150,053	56.5	93,423	35.2	22,207	8.4	265,683
<u>Cherokee</u>							
United States	6,900	21.9	24,329	77.3	260	0.8	31,489
Oklahoma	5,919	20.0	23,440	79.2	251	0.8	29,610
North Carolina	934	66.4	469	33.4	3	0.2	1,406

Source: U.S. Bureau of the Census (1915:31, table 12; 33, table 14)

Table 7. American Indian and United States, Oklahoma, and North Carolina Cherokee Full Bloods, 1910

	<u>Full-Tribal</u>		<u>Mixed-Tribal</u>		<u>Unknown Tribal</u>		<u>Total</u>
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	
<u>American Indian</u>	139,289	92.8	10,251	6.8	513	0.3	150,053
<u>Cherokee</u>							
United States	6,785	98.3	95	1.4	20	0.3	6,900
Oklahoma	5,827	98.4	72	1.2	20	0.3	5,919
North Carolina	920	98.5	14	1.5	0	0.0	934

Source: U.S. Bureau of the Census (1915:38, table 19; 41, table 22).

Table 8. American Indian and United States, Oklahoma, and North Carolina Cherokee Mixed-Bloods, 1910

	<u>White/Indian</u>		<u>Black/Indian</u>		<u>Black/White/ Indian</u>		<u>Unknown</u>		<u>Total</u>
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	
<u>American Indian</u>	88,030	94.2	2,255	2.4	1,793	1.9	1,345 ^a	1.4	93,423
<u>Cherokee</u>									
United States	23,510	96.6	299	1.2	165	0.7	355	1.5	24,329
Oklahoma	22,722	96.9	278	1.2	86	0.4	354	1.5	23,440
North Carolina	429	91.5	2	4.3	38	8.1	0	0.0	469

^aIncludes other mixtures.

Source: U.S. Bureau of the Census (1915:31, table 12; 132, table 51).

Table 9. American Indian and United States, Oklahoma, and North Carolina Cherokee of White Mixed-Blood, 1910

	<u>Below 50%</u>		<u>50%</u>		<u>Over 50%</u>		<u>Unknown</u>		<u>Total</u>
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	
<u>American Indian</u>	18,169	20.6	24,353	27.7	43,937	49.9	1,571	1.8	88,030
<u>Cherokee</u>									
United States	1,702	7.2	2,294	9.8	19,384	82.5	130	0.6	23,510
Oklahoma	1,429	6.3	2,172	9.6	18,997	83.6	124	0.5	22,722
North Carolina	192	44.8	39	9.1	198	46.2	0	0.0	429

Source: U.S. Bureau of the Census (1915:35, table 15; 36, table 17).

Table 10. American Indian and United States, Oklahoma, and North Carolina Cherokee of Black Mixed-Blood, 1910

	<u>Below 50%</u>		<u>50%</u>		<u>Over 50%</u>		<u>Unknown</u>		<u>Total</u>
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	
<u>American Indian</u>	717	31.8	729	32.3	780	34.6	29	1.3	2,255
<u>Cherokee</u>									
United States	51	17.1	58	19.4	190	63.5	0	0.0	299
Oklahoma	49	17.6	53	19.1	176	63.0	0	0.0	278
North Carolina	1	50.0	1	50.0	0	0.0	0	0.0	2

Source: U.S. Bureau of the Census (1915:38, table 18; 132, table 51).

Table 11. Fecundity, Fertility and Vitality of Full-Blood and Mixed-Blood American Indian Marriages,^a 1910

	Children		
	<u>% None</u>	<u>Average Born</u>	<u>Percent Surviving</u>
American Indian Marriages	8.6	4.8	74.7
Full-Blood ^b	10.4	4.5	70.0
Mixed-Blood ^c	6.9	5.3	77.8
Oklahoma Cherokee Marriages
Full-Blood ^b	7.3	3.7	71.8
Mixed-Blood ^c	6.4	3.5	82.0

^a Women between fifteen and forty-four years of age, married at least one year, and neither divorced nor widowed nor married more than once.

^b Husband and wife of same tribe.

^c Both husband and wife of white and Indian blood.

Source: U.S. Bureau of the Census (1915:157, table 53; 158, tables 55 and 56; 160).

Table 12. American Indian and United States, Oklahoma, and North Carolina
Cherokee Full and Mixed-Bloods Under Twenty Years of Age, 1910

	<u>Full Bloods</u>			<u>Mixed-Bloods</u>		
	<u>Total</u>	<u>Under Twenty</u>		<u>Total</u>	<u>Under Twenty</u>	
		<u>N</u>	<u>%</u>		<u>N</u>	<u>%</u>
<u>American Indian</u>	150,053	67,129	44.7	93,423	58,553	62.7
<u>Cherokee</u>						
United States	6,900	3,075	44.6	24,329	15,810	65.0
Oklahoma	5,919	2,684	45.3	23,440	15,268	65.1
North Carolina	934	378	40.5	469	300	64.0

Source: U.S. Bureau of the Census (1915:58, table 39; 132, table 51).

Table 13. American Indian and United States, Oklahoma, and North Carolina Cherokee Populations by Blood, 1930

	<u>Full Blood</u>		<u>Mixed-Blood</u>		<u>Blood Not Reported</u>		<u>Total</u>
	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	<u>N</u>	<u>%</u>	
<u>American Indian</u>	153,953	46.3	141,101	42.5	37,363	11.2	332,397
<u>Cherokee</u>							
United States	8,047	17.8	36,844	81.4	347	0.8	45,238
Oklahoma	7,091	17.3	33,634	82.2	179	0.4	40,904
North Carolina	759	38.7	1,204	61.3	0	0.0	1,963

Source: U.S. Bureau of the Census (1937: 80-81, table 12).

Table 14. American Indian and United States Cherokee Full and Mixed-Bloods
Under Twenty Years of Age, 1930

	<u>Full Bloods</u>			<u>Mixed-Bloods</u>		
	<u>Total</u>	<u>Under Twenty</u>		<u>Total</u>	<u>Under Twenty</u>	
		<u>N</u>	<u>%</u>		<u>N</u>	<u>%</u>
American Indian	153,933	70,786	46.0	141,101	82,830	58.7
United States Cherokee	8,047	3,383	42.0	36,844	21,669	58.8

Source: U.S. Bureau of the Census (1915:96; 103, table 21).

Table 15. Cherokee Population of States: 1910 to 1930

<u>State</u>	<u>Population</u>	
	<u>1910</u>	<u>1930</u>
Alabama	9	287
Arizona	4	101
Arkansas	0	180
California	34	258
Colorado	12	76
Florida	0	5
Georgia	0	15
Idaho	8	36
Illinois	0	18
Indiana	0	10
Iowa	0	26
Kansas	71	191
Louisiana	1	12
Massachusetts	0	8
Michigan	3	96
Minnesota	2	5
Mississippi	0	6
Missouri	13	88
Montana	12	41
Nebraska	0	25
Nevada	0	9
New Jersey	0	7
New Mexico	1	61
New York	5	26
North Carolina	1,406	1,963
North Dakota	34?	4?
Ohio	15	29
Oklahoma	29,610	40,904
Oregon	19	126
Pennsylvania	50	18
South Carolina	87?	2?
South Dakota	7	13
Tennessee	45	38
Texas	0	117
Virginia	19	268
Washington	8	82
Wisconsin	14	40
Wyoming	0	13
Other States	0	11
Total	31,489	45,238

Source: U.S. Bureau of the Census (1937:59-60, table 9).

Table 16. Percent of "Indian Blood" in The
Eastern Band of Cherokee, 1924 and 1957

<u>Blood Quantum</u>	<u>% 1924^a</u>	<u>% 1957^b</u>
4/4	20.1	22.9
3/4	12.8	25.9
1/2	7.2	20.0
1/4	7.1	16.3
Less than 1/14	52.2 ^c	15.0

^aFrom Baker roll.

^bEstimated from school population.

^cSome 24.1 percent were 1/16, and 28.1 percent less than 1/16.

Sources: Gulick (1960:17, table 1), Kupferer (1968:90, table 1).

Appendix

Required Minimum Blood Quantum for Tribal Membership and Population Size of Federally Recognized American Indian Tribes Today

<u>Tribe</u>	<u>Required Blood Quantum</u>	<u>Population</u>	
		<u>1981</u>	<u>Circa 1985</u>
Absentee Shawnee Tribe of Indians of Oklahoma	1/4	1,971	..
Acoma Pueblo	1/4	3,586?	..
Ak-Chin Indian Community	1/4	..	495
Agua Caliente Band of Mission Indians	1/8 ^b	218	210
Alabama-Quassarte Creek Tribal Town	NMR ^b
Alturas Rancheria	1/4	21	20
Apache (Kiowa-Apache)	1/4	833	..
Apache Tribe of the Mescalero Reservation	1/4	2,649	..
Arapaho (Wind River)	RNK ^c	3,375	..
Assiniboine and Sioux Tribes of the Fort Peck Reservation	1/4	..	8,318?
Bad River Band of the Lake Superior Tribe of Chippewa Indians of Wisconsin	NMR	..	3,122?
Barona Group of Capitan Grande Band of Mission Indians	RNK	350	304
Bay Mills Indian Community	1/4	..	840?
Berry Creek Rancheria	NMR	200	185
Big Bend Rancheria	RNK	6	..
Big Lagoon Rancheria	RNK	..	11
Big Pine Valley Band of Owens Valley Paiute-Shoshone Indians	RNK	441	..
Big Sandy Association	RNK	..	131
Big Valley (Rancheria)	1/2	200	300
Blackfeet Tribe	1/4	..	12,879?
Blue Lake (Rancheria)	RNK	25	..
Bridgeport Indian Colony	1/4	70	83
Buena Vista (Rancheria)	RNK	2	1
Burns Paiute	1/4	205	..
Cabazon Band of Mission Indians	1/4	38	21
Cachil Dehe Band of Wintun Indians of the Colusa Indian Community	1/4	25?	18
Caddo Indian Tribe of Oklahoma	1/8	2,031	..
Cahto Indian Tribe of Laytonville Rancheria	1/4	..	121
Cahuilla Band of Mission Indians	RNK	168	148
Campo Band of Mission Indians	1/4	78	213
Cayuga Nation	RNK	380	..
Cedarville Rancheria	NMR	15	10
Chemehuevi Indian Tribe	NMR	15	10
Cher-ae Heights Indian Community of Trinidad Rancheria	1/8	..	63
Cherokee Nation of Oklahoma	NMR	63,000?	64,300
Cheyenne-Arapaho Tribes of Oklahoma	1/4	7,677	..
Cheyenne River Sioux Tribe	1/4	..	9,361?
Chickasaw Nation of Oklahoma	NMR	11,780?	..

Appendix (Cont.)

Chicken Ranch (Rancheria)	RNK	30	34
Chippewa Cree Tribe of the Rocky Boy's Reservation	NMR	..	2,920
Chitimacha Tribe of Louisiana	1/16	260	..
Choctaw Nation of Oklahoma	NMR	31,000?	..
Citizen Band of Potawatomi Indians of Oklahoma	1/8	11,011	..
Cloverdale (Rancheria)	RNK	2	55
Cochiti Pueblo	1/4	954?	..
Cocopah Tribe	1/4	..	800
Coeur d'Alene Tribe	1/4	1,200	..
Colorado River Indian Tribes	1/4	..	2,707?
Comanche Indian Tribe	1/4	7,413	..
Confederated Salish and Kootenai Tribes of the Flathead Reservation	1/4	6,210	..
Confederated Tribes of Coos, Lower Umpqua and Siuslaw Indians	RNK	800	..
Confederated Tribes of Siletz Indians	1/8	1,550	..
Confederated Tribes of the Chehalis Reservation	1/4	377	..
Confederated Tribes of the Colville Reservation	1/4	..	6,636
Confederated Tribes of the Goshute Reservation	NMR	367	..
Confederated Tribes of the Grande Round Community	1/16	1,100	..
Confederated Tribes of the Umatilla Indian Reservation	1/4	1,342	..
Confederated Tribes of the Warm Springs Reservation in Oregon	1/4	2,400	..
Cortina Rancheria	1/4	..	101
Coushatta Community (Allen and Jefferson Davis Parishes)	RNK	310	..
Covelo Indian Community	NMR	2,400?	1,185
Cow Creek Band of Umpqua Indians	RNK	700?	..
Coyote Valley Band of Pomo Indians	RNK	..	225-250?
Creek Nation of Oklahoma	NMR	60,000?	..
Crow Creek Sioux Tribe of Fort Thompson	1/4	..	3,075?
Crow Tribe of Indians	1/4	..	6,961?
Cuyapaibe Band of Mission Indians	1/8	6	5
Death Valley Timbi-Sha Shoshone Band	RNK	199	213
Delaware Tribe of Indians of Western Oklahoma	1/8	..	955
Devils Lake Sioux Tribe	1/4	..	3,300?
Dry Creek Rancheria	NMR	127	130
Duckwater Shoshone Tribe	1/8	325	..
Eastern Band of Cherokee	1/16	8,381	..
Eastern Shawnee Tribe of Oklahoma	NMR	1,327?	..
El-Em Indian Colony (Sulphur Bank)	1/8	..	152
Elk Valley (Rancheria)	RNKK	80	..
Ely Indian Colony	1/4	312	..
Enterprise Rancheria	RNK	65	18

Appendix (Cont.)

Flandreau Santee Sioux Tribe	1/4	..	504?
Forest County Potawatomi Community	1/4	..	683?
Fort Belknap Indian Community	1/4	..	4,608?
Fort Bidwell Indian Community	1/4	199	196
Fort Independence Indian Community	1/16	65	196
Fort McDermitt Paiute and Shoshone Tribe	1/4	529	..
Fort McDowell Mohave-Apache Community	1/4	..	520?
Fort Mojave Tribe	1/4	..	788?
Fort Sill Apache Tribe	1/8	272	..
Greenville (Rancheria)	RNK	150	50
Gila River Indian Community	1/4	..	11,000?
Grindstone Indian Rancheria	1/4	87	113
Hannahville Indian Community	1/2	..	399?
Havasupai Tribe	1/2	..	533?
Hoh Indian Tribe	1/4	101	..
Hopi Tribe	1/2	..	5,063?
Hopland Nokomis Association	RNK	125	219
Hoopa Valley Tribe	1/4	1,598	1,476
Hualapai Tribe	1/4	..	1,375?
Houlton Maliseet Band	RNK	600	..
Inaja-Cosmit (Reservation)	RNK	13	10
Iowa Tribe	NMR	2,118	..
Iowa Tribe of Oklahoma	1/4	286	..
Isleta Pueblo	1/2	3,224	3,401
Jackson Rancheria	RNK	30	25
Jamul Diegueno	1/2	62	23
Jamestown Band of Clallam Indians	1/4	150?	..
Jemez Pueblo	1/4	2,227	2,372
Jicarilla Apache Tribe	3/8	2,308	2,418
Karuk Tribe	RNK	2,000	..
Kaibab Band of Paiute Indians	1/4	..	250?
Kalispel Indian Community	1/4	185	..
Kashia Band of Pomo Indians of Stewarts Point Rancheria	NMR	..	194
Kaw Indian Tribe of Oklahoma	NMR	781	..
Kialegee Tribal Town	1/2
Keweenaw Bay Indian Community (L'Anse Reservation)	1/4	..	3,135?
Kickapoo Tribe in Kansas	1/4	1,198	..
Kickapoo Tribe of Oklahoma	1/4	1,528	..
Kiowa	1/4	7,948	..
Kootenai Tribe of Idaho	1/4	65	..
La Jolla Band of Mission Indians	1/8	420	280
La Posta Band of Mission Indians	NMR	15	18
Lac Courte Oreilles Band of Lake Superior Chippewa Indians	1/4	..	3,500?
Lac du Flambeau Band of Lake Superior Chippewa Indians	1/4	..	1,972?
Laguna Pueblo	1/4	5,020	..
Las Vegas Tribe of Paiute Indians	1/4	105	..
Lookout Rancheria	RNK	..	16
Los Coyotes Band of Mission Indians	RNK	131	208

Appendix (Cont.)

Lovelock Paiute Tribe	1/4	163	..
Lower Brule Tribe of the Lower Brule Reservation	1/4	..	2,643?
Lower Elwah Tribal Community	1/4	403	..
Lower Sioux Indian Community	NMR	..	595?
Lummi Tribe of Indians	1/4	1,225	..
Makah Indian Tribe	NMR	1,789?	..
Manchester Band of Pomo Indians	NMR	232	438
Manzanita Band of Mission Indians	1/16	50	54
Mashantucket Pequot	NMR	350?	..
Menominee Tribe	1/4	..	6,263?
Mesa Grande Band of Mission Indians	1/8	286	327
Metlakatla Indian Community (Alaska)	RNK
Miami Tribe of Oklahoma	NMR	..	1,241?
Miccosukee Tribe of Indians of Florida	1/2	275	..
Middleton Rancheria	RNK	35	62
Minnesota Chippewa Tribe	1/4	..	33,745
Mississippi Band of Choctaw Indians	1/2	3,790	..
Moapa Band of Paiute Indians	1/4	380	..
Modoc Tribe of Oklahoma	RNK	133	..
Montgomery Creek Rancheria	RNK	19	..
Mooretown (Rancheria)	RNK	25	50
Morongo Band of Mission Indians	RNK	718	965
Muckleshoot Indian Tribe	1/8	408	..
Nambe Pueblo	1/4	438	392
Narraganset Community	RNK	1,170	..
Navajo Nation	1/4	..	175,893?
Nez Perce Tribe of Idaho	1/4	2,560	..
Nisqually Indian Community	1/4	175	..
Nooksack Indian Tribe	1/4	425	..
North Fork (Rancheria)	RNK	49	25
Northern Cheyenne Tribe	1/2	..	5,416
Northwestern Band of Shoshone Indians (Washakie)	RNK
Oglala Sioux Tribe of the Pine Ridge Reservation	NMR	..	16,376?
Omaha Tribe of Nebraska	1/2	..	3,876?
Oneida Nation of New York ^a	1/4	469	..
Oneida Tribe of Indians of Wisconsin	1/4	..	8,844?
Onondaga Nation	RNK	1,349	..
Osage Tribe of Indians	NMR	8,768	..
Otoe-Missouri Tribe	1/4	1,450	..
Ottawa and Chippewa Indians of Michigan (Grand Traverse Band)	RNK	..	2,116?
Ottawa Tribe of Oklahoma	NMR	1,548?	..
Paiute Indians of Utah	1/4	543	..
Paiute-Shoshone Indians of the Bishop Community (Owens Valley)	RNK	1,100	1,606
Paiute-Shoshone Indians of the Lone Pine Community (Owens Valley)	RNK	182	196
Paiute-Shoshone Tribe of the Fallon Reservation and Colony	1/4	1,200	..
Pala Band of Mission Indians	1/16	475	549
Papago (Tohono O'Odham)	NMR	..	17,686?

Appendix (Cont.)

Pascua Yaqui Tribe	RNK	..	5,342?
Passamaquoddy Tribe of Maine	RNK	1,993	..
Pauma Band of Mission Indians	1/4	91	107
Pawnee Indian Tribe of Oklahoma	1/4	2,249	..
Pechanga Band of Mission Indians	RNK	215	428
Penobscot Tribe	RNK	..	1,045
Peoria Tribe of Indians of Oklahoma	NMR	3,247	..
Picayune (Rancheria)	RNK	209	32
Picuris Pueblo	1/4	245	210
Pinoleville (Rancheria)	RNK	1	125
Pitt River Home and Agricultural Cooperative Association (X-L Ranch)	1/4	3,031	..
Poarch Band of Creek Indians	RNK	1,743	..
Pojoaque Pueblo	1/4	124	102
Ponca Tribe of Indians of Oklahoma	1/4	2,022	..
Port Gamble Indian Community	NMR	479	..
Potter Valley (Rancheria)	RNK	1	60
Prairie Band of Potawatomi Indians	1/4	3,289	..
Prairie Island Indian Community	NMR	..	293?
Puyallup Tribe	NMR	1,200	..
Pyramid Lake Paiute Tribe of Nevada	NMR	1,120	..
Quapaw Tribe of Indians	NMR	1,878	..
Quartz Valley (Rancheria)	1/2	157	..
Quechan Indian Tribe of the Fort Yuma Reservation	1/4	..	2,182?
Quileute Tribe of Indians	1/2	546	..
Quinault Tribe of Indians	1/4	1,800	..
Ramona Band of Cahuilla Indians	RNK	1	..
Red Cliff Band of Lake Superior Chippewa Indians	NMR	..	2,308?
Red Lake Band of Chippewa Indians	1/4	..	8,104?
Redding Valley (Rancheria)	RNK	42	..
Redwood Valley (Rancheria)	RNK	125	130
Reno-Sparks Indian Colony of Nevada	1/4	507 ^d	..
Resighini Rancheria	RNK	NR ^d	NR
Rincon, San Luiseno Band of Mission Indians	1/8	500	516
Robinson (Rancheria)	1/4	..	183
Rohnerville (Rancheria)	RNK	42	..
Rosebud Sioux Tribe	1/4	..	15,438?
Rumsey Rancheria	NMR	49	29
Sac and Fox Tribe of Indians of Oklahoma	1/4	2,145	..
Sac and Fox Tribe of the Mississippi in Iowa	NMR	..	931?
Sac and Fox Tribes of the Missouri (in Kansas and Nebraska)	1/8	243	..
Saginaw Chippewa Indian Tribe (Isabella Reservation)	1/4	..	891?
St. Croix Chippewa Indians of Wisconsin	1/2	..	543?
St. Regis Band of Mohawks (Akwesasne)	RNK	2,268	..
Salt River Pima-Maricopa Indian Community	1/4	..	4,143?

Appendix (Cont.)

San Carlos Apache Tribe	1/4	..	9,800?
San Felipe Pueblo	1/4	2,151	2,051
San Ildefonso Pueblo	1/4	520	494
San Juan Pueblo	1/4	1,806	1,747
San Manuel Band of Mission Indians	1/8	40	89
San Pasqual Band of Mission Indians	1/8	278	486
Sandia Pueblo	1/4	316	371
Santa Ana Pueblo	1/4	526	552
Santa Clara Pueblo	NMR	1,374	2,622
Santa Rosa Band of Mission Indians	RNK	46	107
Santa Rosa Indian Community Rancheria	1/4	209	297
Santa Ynez Band of Mission Indians	1/3	202	..
Santa Ysabel Band of Mission Indians	RNK	217	553
Santee Sioux Tribe of Nebraska	NMR	..	2,187?
Santo Domingo Pueblo	1/4	2,890	3,260
Sauk-Suiattle Indian Community	1/4	220	..
(Original) Sault Ste. Marie Band of Chippewa Indians, Inc.	NMR	..	11,864?
Seminole Nation of Oklahoma	NMR	9,000?	..
Seminole Tribe of Florida	1/4	1,218	..
Seneca-Cayuga Tribe of Oklahoma	NMR	2,105?	..
Seneca Nation	RNK	5,513	..
Shakopee Mdewakanton Sioux Community (Prior Lake)	1/4	..	86?
Sherwood Valley Rancheria	1/4	231	239
Shingle Springs	1/16	75	257
Shoalwater Bay Indian Tribal Organization	1/4	101	..
Shoshone (Wind River)	RNK	7,088	..
Shoshone-Paiute Tribes of the Duck Valley Reservation	1/4	1,635	..
Shoshone-Bannock Tribes of the Fort Hall Reservation	1/2	3,100	..
Sisseton-Wahpeton Sioux Tribe	1/8	..	8,000?
Skokomish Indian Tribe	1/4	501	..
Skull Valley (Reservation)	NMR	61	..
Smith River (Rancheria)	RNK	130	..
Sokaogon Chippewa Community (Mole Lake)	NMR	..	1,190?
Soboba Band of Mission Indians	RNK	390	593
Southern Ute Tribe	1/4	1,096	..
Spokane Tribe	1/4	1,938	..
Squaxin Island Tribe	1/8	290	..
Standing Rock Sioux Tribe	1/4	..	9,613?
Stillaguamish Indian Tribe	1/16	153	..
Stockbridge-Munsee Community	1/4	..	1,346?
Sugumish Indian Tribe	1/8	583	..
Susanville (Reservation)	1/4	175	116
Summit Lake Paiute Tribe, Nevada	1/4	66	..
Swinomish Indian Tribal Community	NMR	495	..
Sycamore Valley Association (Cold Springs)	RNK	56	205
Sycuan Band of Mission Indians	1/8	55	72
Table Bluff (Rancheria)	RNK	88	135

Appendix (Cont.)

Table Mountain (Rancheria)	RNK	60	68
Taos Pueblo	1/4	1,951	1,803
Te-Moak Bands of Western Shoshone Indians (Battle Mountain, Elko and South Fork)	1/4	1,726	..
Tesuque Pueblo	1/4	312	311
Thlopthiocco Tribal Town	NMR
Three Affiliated Tribes of the Fort Berthold Reservation	1/4	..	7,341?
Tonawanda Band of Seneca Indians	RNK	998	..
Tonkawa Tribe of Indians of Oklahoma	NMR	182	..
Tonto Apache, Payson	1/4	..	63?
Torres-Martinez Band of Mission Indians	RNK	215	343
Tulalip Tribes	NMR	950	..
Tule River Indian Tribe	NMR	549	579
Tunica-Biloxi Indian Tribe	RNK	200	..
Tuolumne Band of Me-Wuk Indians	1/4	102	..
Turtle Mountain Band of Chippewa Indians	1/4	..	2,400?
Tuscarora Nation	RNK	667	..
Twentynine Palms Band of Mission Indians	1/4	13	11
United Keetoowah Band of Cherokee Indians of Oklahoma	NMR	7,500?	..
Upper Lake Band of Pomo Indians	NMR	109	154
Upper Sioux Community	NMR	52?	..
Upper Skagit Indians	1/8	215	..
Ute Indian Tribe of Uintah and Ouray	NMR	1,720	..
Ute Mountain Ute Tribe	1/2	1,528	1,498
Utu Utu Gwaitu Paiute	1/4	101	..
Viejas (Baron Long) Group of Capitan Grande Band of Mission Indians	RNK	183	204
Walker River Paiute Tribe of Nevada	1/4	1,100	..
Washoe Tribe of Nevada and California (Carson Colony, Dresserville Colony and Woodfords Community)	1/4	2,332	..
White Mountain Apache Tribe	1/4	..	8,841?
Wichita Indian Tribe of Oklahoma	1/4	996	..
Winnebago Tribe of Nebraska	1/4	..	2,923?
Winnemucca Colony	1/4	30	..
Wisconsin Winnebago Tribe	1/4	..	3,347?
Wyandotte Tribe of Oklahoma	NMR	..	3,545?
Yakima Indian Nation	NMR	6,775	..
Yankton Sioux Tribe of Indians	1/4	..	5,260?
Yavapai-Apache Indian Community (Camp Verde)	1/2	..	11,000?
Yavapai-Prescott Community Association	1/4	..	110?
Yerington Paiute Tribe (Campbell Ranch)	1/4	363	..
Yomba Shoshone Tribe	1/2	95	..
Yurok Tribe of California (Hoopa Valley)	1/2	3,800	..

Appendix (Cont.)

Zia Pueblo	1/4	650	590
Zuni Pueblo	1/4	5,704	..

^aUnorganized.

^bNo minimum requirement.

^cRequirement not known or not specified.

^dNo residents.

Sources: U.S. Bureau of Indian Affairs (1986a, 1986b).

Comments on Russell Thornton paper
Quantitative Approaches to the History of the American Indian,
Newberry Library
March 1987

One of the explicit assumptions in this conference is that quantifiers speak a common language. Few of us who are commentators are students of the American Indian. Instead, we are linked to the papers by sources, topics and even disciplines. When approached about commenting on a census-based study by a sociologist, I guessed that writing a commentary would be simple. Part of my graduate work was in demography and I've done several large census studies. Thus, I was more than a little distressed by my difficulty with Professor Thornton's essay.

Certainly the paper raised many interesting issues. I learned a great deal about blood quantum requirements for different tribal affiliations. I found out more about Cherokee history than I had known. The essay presented differential fertility rates between full-blood and mixed-blood marriages as well as disturbing differentials in mortality rates. It introduced me to particular strategies for cultural survival that integrated both political and demographic solutions.

Yet, even after re-reading the essay, I was left with a series of questions, concerns, and even some contradictions. Despite all the observations in the paper, conclusions never really emerged. For example, the essay presents data suggesting a slight decline in Cherokee population from 1903 to 1910, a rapid rise in population to 1930, and another rapid increase from 1950 to 1970. Elsewhere in the essay, Thornton acknowledges that several factors might account for this unusual demographic pattern: data problems, changing fertility rates, differing definitions of tribal membership, and

changing reasons for acknowledging that identification. However, there is little effort to evaluate which is the most important for understanding the trends he outlines.

One might hypothesize that at least the 1903 figure is incorrect. Census figures (US, state and BIA) are notoriously filled with inaccuracies and regularly marred by incomplete data, particularly for minority groups within the American population. Some direct evaluation of the accuracy of the censuses would have been useful for understanding whether an actual drop in population occurred or whether the population pattern was, in reality, more consistent.

Another way of understanding the fluctuation (and thereby evaluating the sources) would have been to make use of more of the methodological tools typically employed by demographers. Thorton uses some of the simpler of these measures such as gross fertility/fecundity rates, but excludes many of the finer tuned measures that could explain far more.

For example, he offers a demographic explanation for the 50% increase in population in the 20 years after 1910. That the increase was a result of natural growth is not an outlandish proposition. Preston and Johansson found high rates of fertility among Southwestern tribes as mortality declined in a similar time period. The total Indian control group showed similar increases in the mixed population. It was a period of national prosperity in which Oklahoma probably shared, at least in the teens. Birth rates frequently increase in periods of prosperity.

My quick manipulation of Thorton's crude numbers also indicated that such an increase was possible. Using the birth and mortality measures presented in the essay and making optimal assumptions (i.e., everyone was married, that there was restricted adult mortality, and that all full-blood Cherokees married other full-blood Cherokees), a simple Lotus spreadsheet showed that there would have been enough surviving

children to account for a 50% increase in population. As I built more information into my equations, however, that increase was harder to explain. By adding age distributions and marriage rates, the probability that the growth came solely from natural factors lowers dramatically. Granted, the only age distributions and marriage rates that I had were for all Indians in Oklahoma, but they suggested the possibility that factors other than demography affected the population change.

Had Thorton looked at numbers such as age-specific fertility we would have had a clearer sense of the nature of the increase. For example, different age distributions between full-bloods and mixed-bloods could, in itself, explain large portions of both the discrepancy between the two groups in both fertility and mortality. Knowing specific age distributions in 1930 would shed light on the population increase in the 1950s and 1960s. If most of the 1910-30 population increase came from births, then the later growth spurt might have less to do with tribal membership requirements than with the post-depression, post-war baby boom. Similarly, higher ages for women marrying during the depression and war years would account for the flattened growth of those years.

When looking at these more finely tuned rates, Thornton's argument might be enhanced by using the Oklahoma population as a whole to see how the Cherokee rates compare to the non-Indian population. In making that comparison, it might be possible to assess some of the impact of broader social and economic trends, factors unfortunately absent from the analysis. Depression and war are obvious factors often considered by demographers studying all populations. So too are other factors such as urban residence, education and income. Particularly in Oklahoma with a large mixed-blood population and a large non-reservation Cherokee population, one could expect that these socio-economic factors that influence the population as a whole would also

influence the demographic patterns presented in the paper. Certainly ethnic historians have found that the more intermarried ethnic groups become, the more their population patterns are susceptible to the same pressures as that of the general populace. This question can't even be considered without looking at the non-Indian population.

By analyzing these demographic changes more rigorously and by using another control group, Thornton could then more convincingly integrate the blood quantum definitions and tribal identification strategies into his demographic argument. To an uninitiated reader like myself, the relationships are just not there. The choice of such different blood quantum requirements presented obviously suggests that different strategies are used by different tribes. How that relates to both demographic trends and specific historical events is totally unclear.

Drawing on the work of ethnic and black historians, I wondered if there were economic and political reasons that influenced formal and informal definitions of tribal membership. Was there a reason in the first decade of the twentieth century for Oklahoma Cherokees to downplay tribal membership? The reason might not be as striking as the German-American move away from German identification during World War I. However, discrimination, might have encouraged underregistration, especially in the first years of Oklahoma's political existence. Again, as a non-Indian historian, I had no facts that would let me know whether that was a reasonable assumption.

In the later years, the opposite may have been true. At least one commentator I read noted that understanding the concept of metis was at least somewhat hard for him because in Oklahoma almost everyone was part Cherokee. If that is true, at least in the popular mind, then claiming Indian heritage takes on a very different meaning, one that the essay leaves unclear, except indirectly in the comparisons with the Carolina

Cherokee. It would hardly correspond to being Irish in Chicago on March 17th, but it is not that of an outsider. That meaning could have political, social, cultural or economic overtones. Unfortunately, there were no guideposts to steer me in any direction.

Finally, there is the need to address the combined demographic/strategic trends from a variety of different perspectives. Ethnic historians have long understood that geographically isolated groups have maintained group identities and traditions that are clearer than those in urban and other mixed-group areas. This is true whether one considers the Carolina Sea Islands or the Amish settlements in Pennsylvania. Similarly large ethnic groups have a better chance of preserving their own traditions than do smaller groups that are forced to interact. At the same time, the larger (and more powerful) the group, the more likely it is to threaten the dominant society. Within these parameters, different ethnic groups have devised different strategies and used a variety of different institutions to maintain, or sometimes forget, ethnic and racial identities.

By drawing on some of these studies for additional questions and frameworks, Thornton might find ways to tie his issues together and to provide a clearer meaning. The effects of intermarriage on group identity is a common theme for ethnic historians. So too is the notion of loyalty to the "old country," an issue that is perhaps quite similar to Thornton's comment that mixed-bloods play an important role in making the maintenance of the full-blood way of life possible.

The potential contribution that such work would have for Indian historians is probably clear to everyone here. Its importance for historical demographers, ethnic historians and social historians is also substantial because the questions are important ones for which they have no definitive answers. Understanding the experience of the

Cherokee and other Indian groups would provide important insights for both American ethnic historians and demographers all over the world. By addressing them directly, using better methodological techniques and exploring the ramifications of his findings, Thornton could move us far in understanding how demography is an integral part of a cultural strategy.

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SEARCHING FOR STRUCTURE:

Linking Manuscript Census Returns to Explore the Social Structure
of the Early Twentieth Century Crow Reservation.

By

Frederick E. Hoxie
Newberry Library

In North America, the social history of native communities follows a common, four-part outline. First, there was a period when groups existed without European contact.¹ Second, there was a period of non-coercive interaction with Europeans during which natives and nonnatives exchanged microbes, information, trade goods and political allegiances. Third there was a coercive period during which Europeans established military, economic and cultural control over the Indians. In the wake of this domination there was a fourth period, at least for those who survived. For those who were not destroyed, the fourth period was marked by survival and cultural persistence.

In the United States, these four stages were usually of common--but uneven--lengths. The precontact period stretched over thousands of years. The stage of non-coercive interaction lasted for a century or more. The period of domination was shorter. Aside from the coastal tribes of the Atlantic seaboard and the Pueblos of the Spanish Southwest, Indian people now living within the borders of the U.S. were not subjected to direct conquest by EuroAmericans until the nineteenth century. And for most of these people, the pace of domination was most rapid during the single

generation bounded by the Mexican War and Sitting Bull's dramatic, but short-lived victory at the Little Big Horn.

The swiftness and brevity of the domination process is probably one of the reasons why so many tribal cultures have survived into the twentieth century. Like the citizens of Hiroshima or Nagasaki in August, 1945, Indian people witnessed levels of destruction and dislocation that were unprecedented and therefore unforeseen. Communities saw their subsistence base, their religious systems, and their political leadership assaulted and crushed before their eyes. On the Northern Plains, even groups who responded peacefully to the American onslaught were swept into the maelstrom, losing their freedom, their military power, and their economic resources. But because this destruction happened almost overnight, the survivors did not have memories of generations of decline. Instead, like the survivors of the atomic attacks on Japan, they emerged into the new age of the reservation with tribal memories and cultural goals intact. They remembered earlier periods in their history and they were sensitive to the continuities in their lives as well as to the disjunctures.

This familiar overview of American Indian history carries two important consequences for scholars interested in native cultural persistence and the modern history of Indian communities. First, while studies of federal policy formulation and administration provide a valuable backdrop to the story, they cannot explain the cultural survival and growth we have witnessed in our own time. We need to understand the sources and nuances of governmental action, but we also need to recall that for Indians, government agents and

their regulations were like boulders before a swift canoe--large, grey objects to be avoided as one maneuvers downstream.

Persistence is a dynamic process. The history of persistent communities therefore involves more than the survival of fixed traditions. Communities that maintained a cohesive identity and an array of cultural institutions were active agents of their own survival. They carefully balanced local needs against external forces. The history of that process requires an analysis of native strategies and native actions.

The second implication of this four-part outline is that cultural persistence is the greatest contribution of Indian people to modern American history. By their very survival, native communities threw a monkey wrench into deeply held assumptions about progress and the "march of history." Indians' insistence on the authenticity of their own history and culture first angered, then baffled, and now fascinates social scientists. If I may push my Hiroshima analogy just one step farther, it would seem that just as the recent history of Japan offers an argument against the ability of military might to destroy a people, so the history of American Indians in the twentieth century offers us hope of escaping a future of undifferentiated cultural homogeneity. America's cultural diversity--forced upon the AngloAmerican consciousness by Indians and other ethnic peoples -- is a rich antidote to that homogeneity and a possible key to understanding and managing the cultural and political conflicts that are endemic to the modern era.

So the story of cultural persistence is important--perhaps vital--and it must be understood from the Indian point of view. How have we told this story? In a word, qualitatively rather than quantitatively. For the most part we have focused on "qualities" --attitudes-- rather than concrete data. We--meaning academic scholars--have traced political, religious and economic behavior and extrapolated from this a picture of whether and how people have "acculturated." Among those focused primarily on historical questions, the studies of politics usually stress factionalism, discussing its sources or explaining its absence. Inevitably in these treatments authors begin using terms like "progressives" and "traditionalists" and the texture of native culture becomes lost in a narrative rooted in an externally derived typology.² The literature on religious change is similarly concerned with ideological divisions and confined to discussions of attitudes. Robert Berkhofer Jr.'s Salvation and the Savage, for example, --a classic work which opened a range of new questions for scholars-- runs the risk of distorting our picture of tribal history by focusing on schisms and emphasizing externally defined doctrine rather than internally articulated belief.³

The literature on specific tribes usually follows this qualitative trend. For the Crows of Montana, the most prominent work on acculturation is by anthropologist Fred Voget. Voget is the author of the recent--and excellent-- The Shoshoni Crow Sun Dance and a number of shorter articles on culture change. One of these, entitled "Crow Sociocultural Groups," argues that "the contact of the individual with alternative values in the process

of acculturation serves to induce conflict and ... is followed by attempts on the part of individuals and groups to find an adequate adjustment." The product of this adjustment process, Voget asserts, is an array of four personality "types."⁴ He goes on to describe these four types without reference to systematic analysis of Crow behavior or to particular historical events. Another observer with a different set of presuppositions might define three or five or six personality types. More important, the discussion of attitudes leads us away from, rather than towards, a clear picture of how it is that a century after the U.S. government set out to eradicate Crow traditions those traditions are both viable and appealing to the tribe. The only assumption one can make is that persisting attitudes are somehow imprinted in the tribal mind or determined through some system of intellectual deliberation and choice. Certainly there is value in a qualitative approach. We have to take seriously the things Indian people say and do, but if we recall that we are interested in a historical process in which individuals and groups acted to defend and adapt their cultures in the face of an unprecedented alien onslaught, we need to search a bit deeper and to devise a way of understanding native actions on their own terms. We need to search for the structure of the community as well as its rhetoric.

The wealth of census data available for the Crow reservation provides an alternative--or a complement--to qualitative analyses like Voget's. Starting with the establishment of the modern Crow Agency in 1884, the Bureau of Indian Affairs conducted an annual census of the tribe. Beginning in 1887 (earlier records were

apparently lost) we have an annual enumeration of individuals by "family" that includes their birthdate and a description of their relationship to the family head. In addition, the U.S. Bureau of the Census conducted two special enumerations of all Indian communities that have survived in manuscript form. The manuscript returns for the special Indian enumerations of 1900 and 1910 are available on microfilm and contain more than twenty categories of information ranging from birthdate to education to information on education and ancestry. Finally, the local agency superintendent conducted a variety of irregular enumerations that contain information not available elsewhere. Similar records exist for all other jurisdictions in the United States, so an examination of Crow materials could provide a guide for studies of other native communities.

But how can census data provide a picture of cultural persistence? Actually, a census by itself cannot. What makes the Indian censuses useful to us is their number, the wealth of "extra" questions they contain (particularly the special enumerations of 1900 and 1910), and the fact that very few tribal members left the reservation before World War II. (Indeed, for most of the early twentieth century Crows could be jailed for leaving their communities without the permission of their agent.) Thus we can link a number of enumerations to provide a picture of specific changes in community life.

Before proceeding, I should distinguish between linking data and comparing them. Comparing data occurs when the same measure is used at two different points to examine an aspect of society at

two moments. For example, we can compare the number of male Crow children under the age of 10 in 1900 and 1910 and say that this component of the population increased roughly 5% in a decade (from 393 to 412; See Tables 4 and 5). Linking these measures is not possible since all the Crow children under ten in 1900 were over ten in 1910. We can link the 1900 and 1910 censuses however, by asking questions regarding the experience of a group which could be identified in both censuses. We might ask, for example, how many of the children under 10 in 1900 were alive in 1910? (The answer is 329, a decline of 16%.) Or, what level of education did they attain? Or did they marry relatively early or late? The same kind of questions can be asked of individuals. Where did they live? Did they move? If so, where? Who did they marry? Where did their children live? and so on. Linking census returns thus allows us to grasp the perspective of the Indians who were at the center of the persistence effort. While data alone cannot offer an explanation, they do offer an opportunity--to return to an earlier metaphor--to stop examining tribal history from the vantage point of those big, grey, BIA boulders or from the perspective of the shoreline, shouting back and forth to the pilot of the tribe's moving "canoe of state." Instead we can begin to imagine the story from within the boat itself.

While census data offer us a wealth of information and a variety of indices of cultural persistence, I will focus the remainder of my presentation on two areas that are crucial to any picture of Crow community organization and cultural persistence: residence and marriage. Residence is the process by which people

came to settle in a fixed location. Where did people settle when the reservation was established? Who settled with them? What kinds of structures emerged in their new settlements? Marriage is the process by which people select mates and form families. Who did people marry and for how long did they remain together? What did Crows believe about marriage and family formation? While cold numbers and lists can't answer these questions completely, they can convey a sense of the community's actions in these areas and they can give us a picture of the structure the Crows created to reflect and perpetuate their cultural beliefs.

Prior to the establishment of modern Crow Agency in 1884, the tribe migrated through an annual subsistence cycle that carried individual Crows from the Wind River range of north central Wyoming to the headwaters of the Missouri River in northwestern Montana. The tribe traces its history to the Hidatsa. Their separation from that Missouri River horticultural tribe continues to be the subject of much study, but the record at present indicates that there were a series of migrations away from the Hidatsa beginning in the seventeenth century, and that the people who left the parent tribe joined forces, developing common social and religious institutions, and eventually coming to think of themselves as Apsaruke, or Crow.

The first extensive written description of Crow contact with Europeans appears in Francois Antoine Larocque's journal of his sojourn on the upper Missouri in 1804 and 1805. Subsequent descriptions by Edwin Thompson Denig, Zenas Leonard and James Beckwourth present a portrait of a people who adapted readily to

the equestrian culture of the nineteenth century plains.⁵ When the Crows acquired a resident agent from the Bureau of Indian Affairs in 1871 they were described as divided into two bands, the River Crows who spent most of the year north of the Yellowstone River, and the Mountain Crows, who wintered near the Big Horn and Wind River ranges and who spent the remainder of the year hunting in the Tongue, Big Horn and Powder River valleys. Other reports indicated that the Mountain Crows, often called the "Main Body" of the tribe, were subdivided with a smaller group, the Kicked in the Bellies, often moving farther south into Wyoming to hunt.

During the 1870s, the Crow Agency was a mountain trading post and ration station where tribesmen would come in the fall to collect their annuities. Some families remained at the agency for the winter, but for the most part the tribe continued to follow the buffalo and other game. As the game declined and raids from other tribes continued, federal agents began discussing the removal of the agency to a "permanent" location. Agent Armstrong wrote in 1882, for example, that "many of the Crows are willing and ready to settle down and roam about no more and to exchange a part of their ponies for cattle and it is of the utmost importance that the question of their permanent location and home should be speedily decided."⁶ And many Crow leaders believed that the time had come to adopt some of the white man's ways. Iron Bull told a visiting Senate investigating committee headed by Henry Dawes that, "All the buffalo and elk and deer are gone, and we have so little to eat that our children are starving. We want the Great Father to give us cattle, and we want cows that will have

young ones, and we will put them at the foot of the mountains and all along the creeks, and by the springs."7 In the spring of 1884 the agency headquarters was moved to the banks of the Little Big Horn River and the modern Crow reservation was established. (See Figure 1)

As tribesmen began to report to the new Crow Agency they faced an unprecedented set of conditions. The most dramatic of these was the requirement that, as "ration Indians," they agree to settle permanently within the boundaries of the new reservation. (These boundaries were diminished by two land sales, the first eliminated the mountainous western portion of the reservation in 1882, and the second --in 1904--caused the loss of a northern portion which stretched to the Yellowstone River.) While stretched across more than than 350 square miles, the reservation represented a confined space where Crows were now to make their homes. What did they do?

To answer this question, I have turned to five tribal enumerations. The first of these, undated but probably written in 1883 or 1884, is a "Record of Indian Bands." This census, contained in four small notebooks contains a list of 401 families. The "head" of each family is listed along with a record of the number of men, women and children in the group. In addition, the census identifies each family according to a list of twenty-seven "bands." The 1884 census thus provides a picture of prereservation social structure. We know that all the people listed by name in that census were the heads of households, and we

know that people grouped together in a "band" were affiliated in some way.

The second document to be employed was the 1900 federal census. While the federal census does not provide information regarding the location of individual residences, it does contain information on spouses, children and marital status. Third was the roster of Crow allotments prepared following the completion of the first general division of the reservation into individual homesteads in 1905. This list indicated the precise location of assigned residences as well as the location of allotments assigned to other family members. Taken together, this information allows us to be fairly certain of an individual residence. Fourth was the 1910 federal census, which contained information parallel to that in the 1900 enumeration, and fifth was a census of "Adult Crow Indians" prepared by the Agency Superintendent in 1920. This list indicated the home district of Crow adults and listed husbands and wives together. As a set, these records provide us with five "snapshots" of Crow society taken at intervals over a 36 year period. While certainly flawed in many respects, the documents can be checked against each other to eliminate many errors. My hypothesis has been that linking individuals in these records should allow me to trace the process by which people shifted from a migratory hunting subsistence to a pattern of permanent residence in an agricultural community.

An initial comparison of the 1884 and 1900 censuses produced a list of 183 individuals who appeared in both. That is there were 183 Crows who were listed as heads of a household prior to the

establishment of the reservation who were still alive in 1900. Of the 183 people who appeared in the first two censuses, 135 were identifiable on the 1905 allotment roll. I labelled these people the "First Families of the Crow Reservation." They and their immediate families can be identified in the federal census, their allotments can be located, and they can be tied to a prereservation social affiliation. Moreover, because they were heads of households throughout the first twenty years of reservation life, they can be reasonably called a sample of the backbone of Crow society in that period. Table 1 presents an initial description of where these first families settled. The bands have been divided according to the three major divisions of the reservation--the Little Big Horn and Big Horn valleys and the areas along Pryor Creek. Several bands (12,19,23,) contained few people who appear in subsequent censuses, but other groups are quite large, most notably 4, 8, and 13. Also, while most of the groups spread over more than one district, there are clear preferences within them. Number 13, for example, settled predominately along Pryor Creek, and Numbers 21 and 22 preferred the Little Big Horn. We will leave aside for the moment a description of what these "bands" were, and trace the first families a bit farther by asking, "How did they act once they settled into life on the reservation?"

Figures 2 and 3 trace the behavior of four families from Band 13 during the first four decades of the reservation era. They present in simple form the location of the first families and their offspring. In Figure 2 we can see that even though Three

Bears left his wife (or she left him) and moved to the Little Big Horn district to marry Old Beaver, he returned to Pryor following her death. Following his departure, his second wife, Twin Woman, married Pretty Coyote and remained in the district. The Back of the Neck family presents a similar picture: following Back of the Neck's departure for Big Horn, his wife, Corn Woman remained at Pryor with their children. In addition, Goes to the House, listed as a member of their family in 1900, also remained in Pryor and married a local man.

Figure 4 describes a similar phenomenon for band 8. Here nine of the band families were traced through time and across the three districts where they made their homes. Three families began in Pryor, four in the Big Horn, and two in the Little Big Horn. only Alex Crane, child of The Crane and Kills With Horses, sought a spouse outside his district, and only Knows War, the son of Little Wolf left his district to take up residence elsewhere.

While the preservation "bands" appear to have had varying degrees of influence in shaping an individual's decision about where to settle initially, it is clear that once located in a district, Crow families persisted. Why? Was there a structure, either in the prereservation culture, or in the emerging reservation community that fostered persistence in these separate districts? A return to Figures 2,3,and 4 provides one hint: in none of these cases--or in any of the dozens of family genealogies I have traced--is there a case of members of one band marrying the offspring of fellow band members. Whatever the bands were, they appear to have been exogamous. Thus, children of the

Crow first families--particularly the men who are most easily traced and therefore overrepresented in the charts--found spouses from outside their "band" but within their district . As a result, there was relatively little movement away from the places where first family members had originally settled. But how did this work? What shaped a person's selection of a marriage partner and what were the ties that linked an individual to other members of his "band." Obviously, these ties varied in intensity since they did not cause groups to stick together during the settling of the reservation. To answer these questions we have to return to the "bands" listed in the 1884 enumeration.

The agent's list gives only the names of family "heads," the person who presumably drew rations and annuities for his group. Thus even though we can see from Table 2 that some "bands" had over 200 members, we have no more than 30 names for each of them. And our first families number less than half the total number of family leaders. Our evidence grows thin and we need to proceed cautiously. Nevertheless, we can learn something about the people who were family heads in 1884. Searching the ethnographies, particularly those by Robert Lowie and Edward Curtis which were based on fieldwork conducted before World War I, we can begin to identify band members according to their clan, warrior society and divisional affiliations.⁸ Table 3 presents this information.

Despite the fact that most scholars agree that the tribe was divided into three major divisions, the written record presents only two of them: the River Crows and Mountain Crows. The River Crows came to the reservation from the north, so it is not

surprising that the bands reflect a division between mountain and river people. Bands 7, 10 and 13 have two or more Mountain Crow family heads, while Band 14 contains two River Crow families. The table also indicates that the River Crow families preferred the northern portion of the reservation, the area near the confluence of the Little Big Horn and Big Horn rivers which came to be called the Black Lodge district. Three of the four River Crow family heads settled in Black Lodge.⁹

Table 3 also suggests that the family leaders within each band may have belonged to the same warrior society. There were two of these and they had long been rivals in acquiring war honors and prestige. Membership in these societies was somewhat fluid, however, so one might not expect to find a camp to be completely homogeneous. There is no evidence that membership in a warrior society would affect marriage decisions.

Finally, Table 3 indicates clan affiliation. Among the Crows clans were matrilineal and exogamous. The tribal term for clan translates as "lodge where there is driftwood," meaning a place where people cling together, and the functions clans played seem to bear out this imagery. Clans provided individuals with sponsors when seeking adoption into a warrior or religious society, and defenders when they suffered wrongs or insults. Despite the sketchy evidence in Table 3, one can see that two general patterns emerge. Bands 10,13,14, and 15 were dominated by one clan. Bands 4,7,8, and 22 appear to be made up of families from different clans. (Band 27 and 5 contain clans that were generally affiliated with one another, and so may fit more readily into the first

group.) These patterns were explained to anthropologist Donald Collier in the 1930s by tribal elders. To quote a recent summary of Collier's work:

Collier found that in winter the two main divisions of the Crow separated into smaller camps for periods of a week to three months. These camps are described in terms of two ideal types, the first type consisting of a clan chief and a number of his clansmen and their families, and the second composed of a prominent leader, a few of his clansmen, and their families, and a larger number of bi-lateral relatives, friends, and unrelated persons. In practice most camps were intermediate between the two types, but Collier felt that his data showed a shift from the clan to the bilateral type during the period from 1860 to 1885.¹⁰

The reservation was established at a time when clan affiliations were being strained by competing associations. This strain was no doubt the product of the warfare and forced migration that was so much a part of Crow life in the 1860s and 1870s. While clan ties continued, people began living and travelling in different--perhaps competing--associations.

Table 3 and Collier's fieldwork may not help us determine why individuals settled where they did, but they do explain why they seem to have been so persistent once they found a new home. With the creation of the reservation, the tribe had a secure area in which to settle. There were many reasons why an individual might

have chosen one district over another, but it is clear that each district was identified with a particular political figure--Plenty Coos at Pryor, Medicine Crow at Lodge Grass, Iron Bull and Pretty Eagle in the Big Horn valley, and Two Leggings and Two Belly at Black Lodge. More important for explaining persistence, however, is the fact that each district contained members of several Crow clans. Certain clans might predominate in a district (Not Mixed or Filth Eating at Black Lodge, Sore Lip or Burnt Mouth at Pryor, Whistling Waters at Lodge Grass), but members of other clans would also be present. Having already experienced mixed bands, residents of the new reservation districts accepted them and settled down. The result was a rural community in which clan ties were preserved and families could survive and grow.

Nothing in the otherwise alien reservation environment directly disrupted the clan system. Moreover the clan system served to enforce the Crows' ties to their new districts by providing a method of regulating marriage in an unfamiliar setting. While there was a great deal of visiting between districts, there was no necessity for children of the reservation's first families to leave home to find a spouse. Crows had long been used to balancing clan and other obligations, but that balance had always been struck in the mobile world of the buffalo hunt. With the emergence of temporary, multi-kin bands in the late nineteenth century, the tribe was able to transfer that balance to the reservation.

A description of family formation and persistence would sound strange to George W. Frost if we were able to transfer him

here from his post as Agent to the Crows in 1877. According to Frost there was no structure to Crow life, and there were certainly no families. "Perhaps the worst feature of the Crow tribes," he wrote in his annual report that year, "is the almost perfect disregard of marital rights. Polygamy is common, a man taking all the wives that he can support, and, in their language, 'throwing them away' at pleasure. ... They consider adultery no crime, and ... for its commission there is no punishment." A decade later as the new Crow Agency boarding school was being established, Mr. Frost's successor Henry E. Williams suggested that a 12 foot high board fence be erected around the new buildings. "There is now only a wire fence," he reported to Washington, so that "every Indian from the camp who wishes to can converse with the pupils and it cannot be prevented." The evil to be avoided was obvious: "the scenes of camp life ... are detrimental to the pupils."¹¹

The statistics in Tables 4 through 15 present a different view. The tribal population, fixed at better than 2500 in 1884, declined precipitously in the first two decades of the reservation era. By 1900 the number of Crows had dipped below 2,000. Tables 4 and 5 indicate that much of the population loss occurred among young people, particularly the first generation born on the modern reservation. Both the 10-19 year old cohort in 1900 and the 20-29 cohort in 1910 are substantially smaller than one would expect in a "normal" population. It would seem reasonable that Crows would be particularly concerned about the fate of their children. Tables 6, 7, and 8 indicate some of the choices Crows made when

they selected spouses and had children. While the percentage of the population reporting "100% Indian blood" declined from 91% to 77% between 1900 and 1910, the tribe continued to be overwhelmingly Crow in ancestry and orientation. 98% of all reservation residents were Crows, and two thirds of them spoke only their tribal language.

When one turns to marriage statistics, there are two striking patterns. First, Crows valued marriage. (I use the term marriage even though I have no doubt that the Crow respondents and the federal census takers had very different definitions of that term. I believe, however, that both understood the concept of a stable, long term union of men and women.) Over 90% of all adult Crows were either married or widowed, according to the federal censuses of 1900 and 1910. Moreover, Crows appear to have married for long periods. Of the 53 people aged 60-69 who were married in 1900, they reported that they had been in this position for an average of more than thirty years.

The second pattern is revealed in Tables 13, 14, and 15. Here we see extraordinary figures on the frequency of marriage. In an age when some divorces required a formal act of the state legislature, 20% of the tribe reported that they had been married four times or more. One might think that the agent's condemnations were at least based on something concrete. But look again. For both men and women the length of last marriage for individuals married in excess of five times--summarized in Tables 14 and 15--indicates that the ideal of a long-term stable union had not been abandoned. Two themes from the tribal ethnographies and individual

autobiographies help explain this juxtaposition of frequent marriage and long term unions. First, one feature of the rivalry between warrior societies was wife kidnapping. This practice -- which generally took place in the spring--was supported by the warrior ethic which prohibited resistance from the husband. And second, Crow young people seemed to participate in a period of experimentation before settling down with a single partner. Elsewhere I have described the government's tribal courts where virtually all of the prosecutions for "adultery" were of men and women under the age of 25.¹²

As they had in the prereservation era, Crow young people experimented widely with possible spouses even as they shared the ideal of a long-term, stable union. Thus, while agents and missionaries condemned their immorality, Crows persisted in their traditional practices and formed the foundation of a new reservation community.

Clearly this modest search for the structure of Crow society in the transition to a reservation environment requires a great deal more work. Four areas in particular strike me as promising:

1. A great deal more can be learned from additional census data. We can discover when people first married and had children. We can learn the impact of schooling on family patterns by correlating school attendance (recorded in the special enumerations of 1900 and 1910) with marriage, childrearing, and occupation patterns.

2. We can trace people further through the use of other forms of enumeration: equipment issue records, records of the agency flour mill, records of cattle ranching operations, etc.

3. We can correct, flesh out, or reject the written records by consulting members of the contemporary Crow community who have special expertise in Crow history. This consultation can help us particularly in searching out clan designations--something missionaries and government agents uniformly ignored.

4. Finally, by building a sense of Crow social structure, we can look for its relationship to Crow political leadership, economic activity and religious life. How did changes in one realm affect or reflect changes in another? Where were the structures being built to buttress the culture, where were there no such structures? Where was the continuity? Where was the change?

While admitting the difficulty of using census data, it seems clear that these enumerations promise to provide a new and exciting source of information about the cultural transformation that took place within the Crow community in the late nineteenth and early twentieth centuries. They give us a picture of

individual decisions and a sense of the pattern within those decisions. They convey not the rhetoric of interaction with the outside world, but the steps people took to maintain and to rebuild a universe of associations and meanings. If we can grasp the structure of that creative process, we may well take a step closer to understanding the dynamic process which created the world around us.

1 This "first part" of the outline is obviously a very long period with its own outline, or list of outlines. By reducing all prehistory into an element in an outline I do not mean to adopt either the "ethnographic present" or to assume stasis in precontact Indian history.

2 See, for example, the discussion of Indian responses to the reservation environment in Arrell Morgan Gibson, The American Indian: Prehistory to the Present (Lexington, Mass.: D.C. Heath, 1980), chapters 17 and 18. Loretta Fowler, Arapahoe Politics, 1851-1978 (Lincoln: U. Nebraska Press, 1982) provides a partial antidote to this literature by describing the relationship between Arapahoe social structure and persisting political practices. Still, the book focuses primarily on political behavior and on what Fred Eggan calls in his foreward "the most successful adjustment to white culture of any Plains tribe." (p.xv)

3 For another example of this school, see Deward E. Walker, Jr., Conflict and Schism in Nez Perce Acculturation: A Study of Religion and Politics (Pullman: Washington State University Press, 1968). An antidote to the Berkhofer/Walker approach is suggested in the introduction and annotations in Raymond J. DeMallie's edited transcripts of Black Elk's teachings published as The Sixth Grandfather (Lincoln: University of Nebraska Press, 1984).

4 Fred Voget, "Crow Socio-Cultural Groups," in Sol Tax, ed., Acculturation in the Americas (Chicago: University of Chicago Press, 1952), p.89. See also Fred Voget, "Adaptation and Cultural Persistence Among the Crow Indians of Montana," in Ernest Schusky, ed., Political Organization of Native North Americans (Washington, D.C.: University Press of America, 1980); and The Shoshoni-Crow Sun Dance (Norman: University of Oklahoma Press, 1984).

5 The classic ethnography of the Crow is Robert H. Lowie, The Crow Indians (New York: Rinehart, 1935). For Larocque, see W. Raymond Wood and Thomas D. Thiessen, editors, Early Fur Trade on the Northern Plains (Norman: University of Oklahoma Press, 1984). See also, Edwin Thompson Denig, Five Indian Tribes of the Upper Missouri, edited by John Ewers (Norman: University of Oklahoma Press, 1961); Zenas Leonard, Narrative of the Adventures of Zenas Leonard, edited by Milo M. Quaife (Chicago: R.R. Donnelley, 1934); James Beckwourth, The Life and Adventures of James P. Beckwourth, Bison edition (Lincoln: University of Nebraska Press, 1972, Reprint of 1856 edition); and W. Raymond Wood, The Origins of the Hidatsa Indians: A Review of Ethnohistorical and Traditional Data (Lincoln: J&L Reprint Co., 1986, Reprint of 1980 edition).

6 H.J. Armstrong to Commissioner of Indian Affairs, October 5, 1882, Item 3, Box 9, Crow Agency Archives, Federal Records Center, Seattle, WA.

7 Senate Report 183, 48 Congress 1 Session, "Report of the Select Committee to Examine the Condition of the Sioux and Crow Indians," Serial 2174, p.3.

8 See Robert H. Lowie, "Social Life of the Crow Indians," American Museum of Natural History Anthropological Papers, Vol. IX, Part 2 (1912); and Edward S. Curtis The North American Indian, 20 Volumes (New York: 1907-1930), Volume IV.

9 There is also some evidence that individuals who settled in Lodge Grass were members of the Kicked in the Bellies division of the tribe while the Mountain Crows, or Main Body settled at Pryor and along the Big Horn river. Lowie noted that "Kicked in the Bellies" "today ... is applied to the Crow Indians of Lodge Grass, though by no means all of them are descendants of the local group." See Lowie, "Social Life of the Crow Indians," p.183.

10 Joseph A. Maxwell, "The Evolution of Plains Indian Kin Terminologies: A Non-Reflectionist Account," Plains Anthropologist, Vol.23, No.79, p.19-20. I am grateful to Peter Nabokov for bringing Collier's field work to my attention.

11 Annual Report of the Commissioner of Indian Affairs, 1877, p.529; and 1887, p.219.

12 On wife kidnapping, see Lowie, The Crow Indians, p.186. On adultery before the tribal courts, see Frederick E. Hoxie, "Towards a 'New' North American Indian Legal History," American Journal of Legal History, Vol.30 (1986), p.351-357.

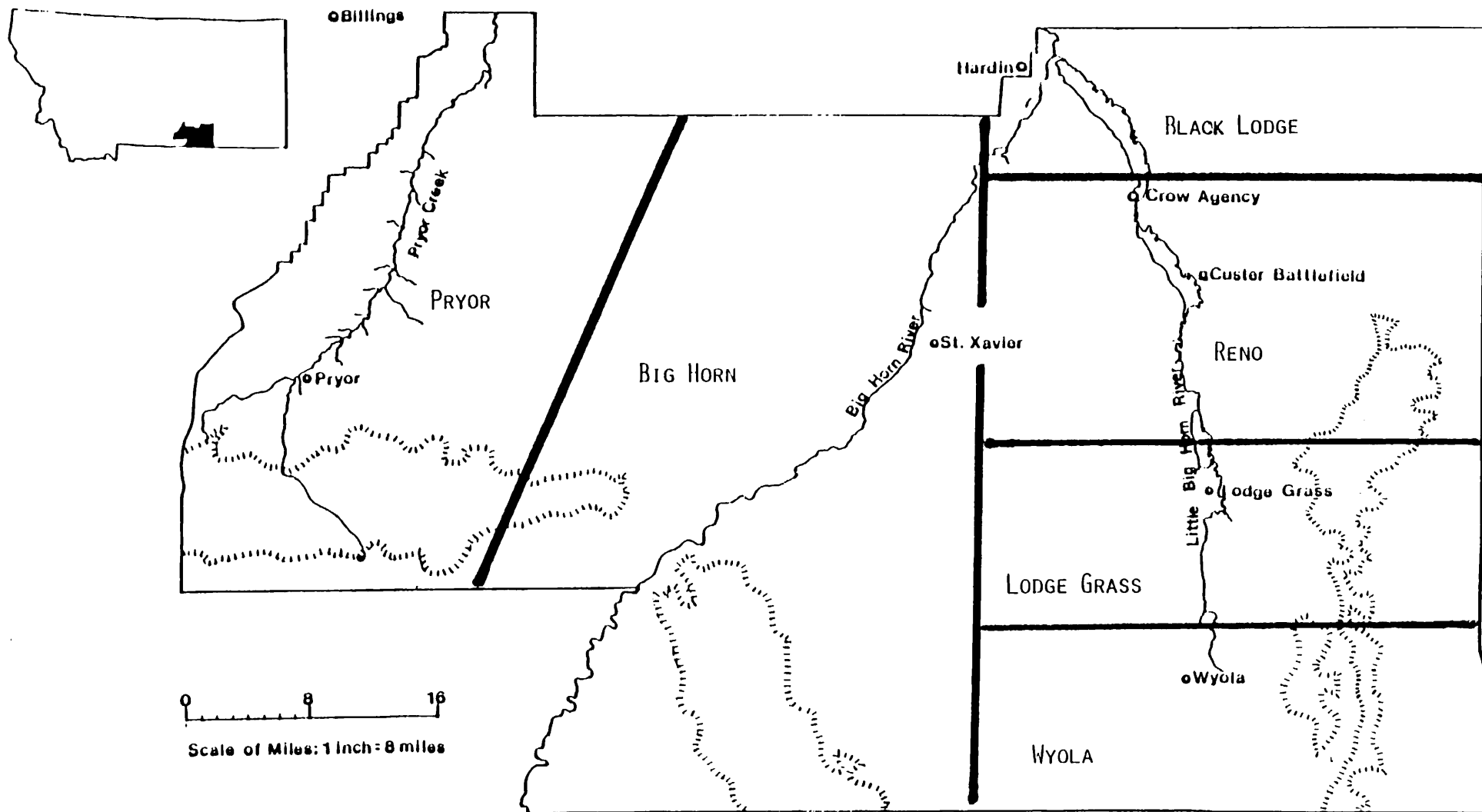


Figure 1

CROW INDIAN RESERVATION:

Boundaries and Principal Settlements after the 1904 Land Sale

FIGURE 2: Plenty Coos First Families

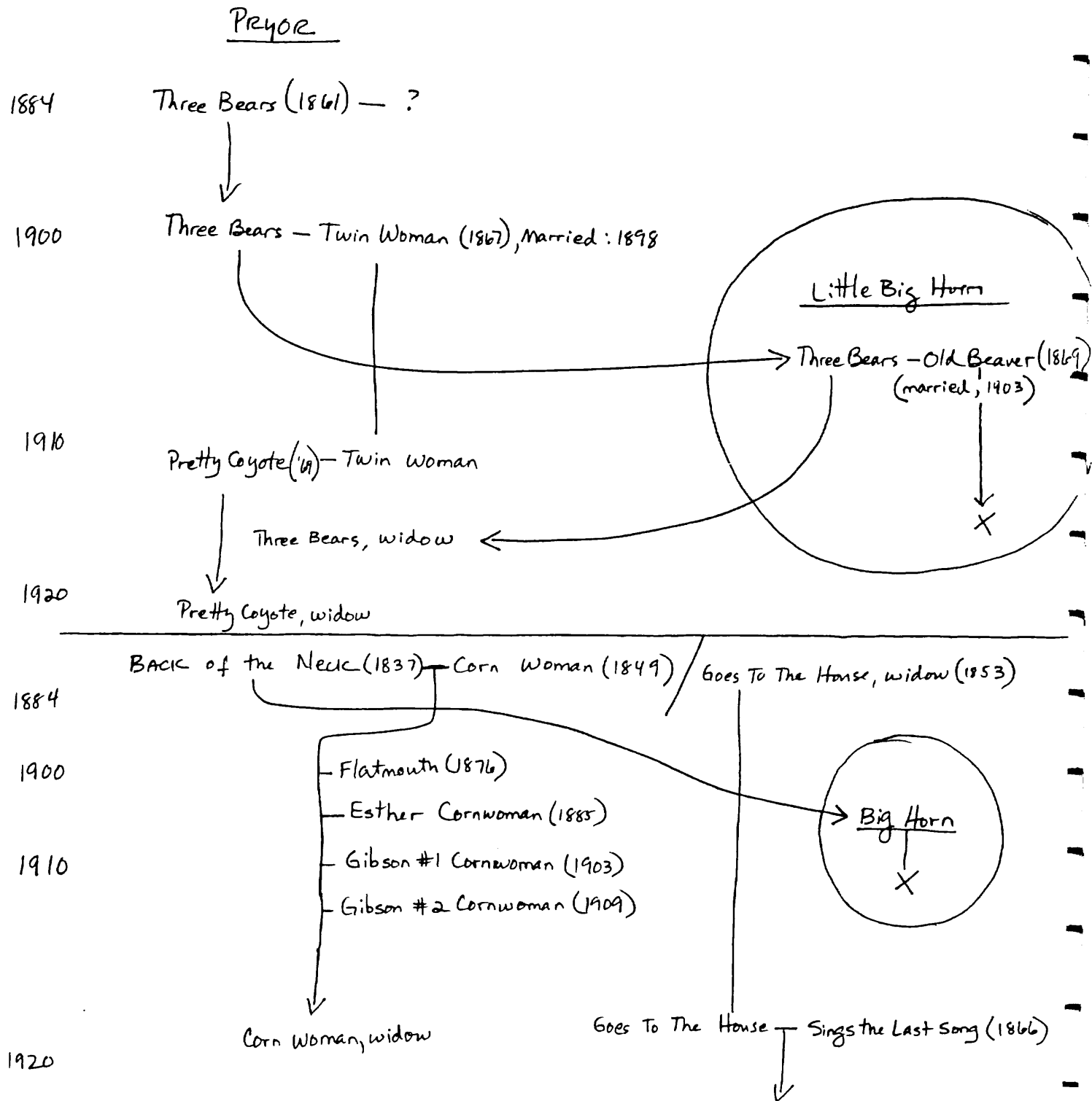
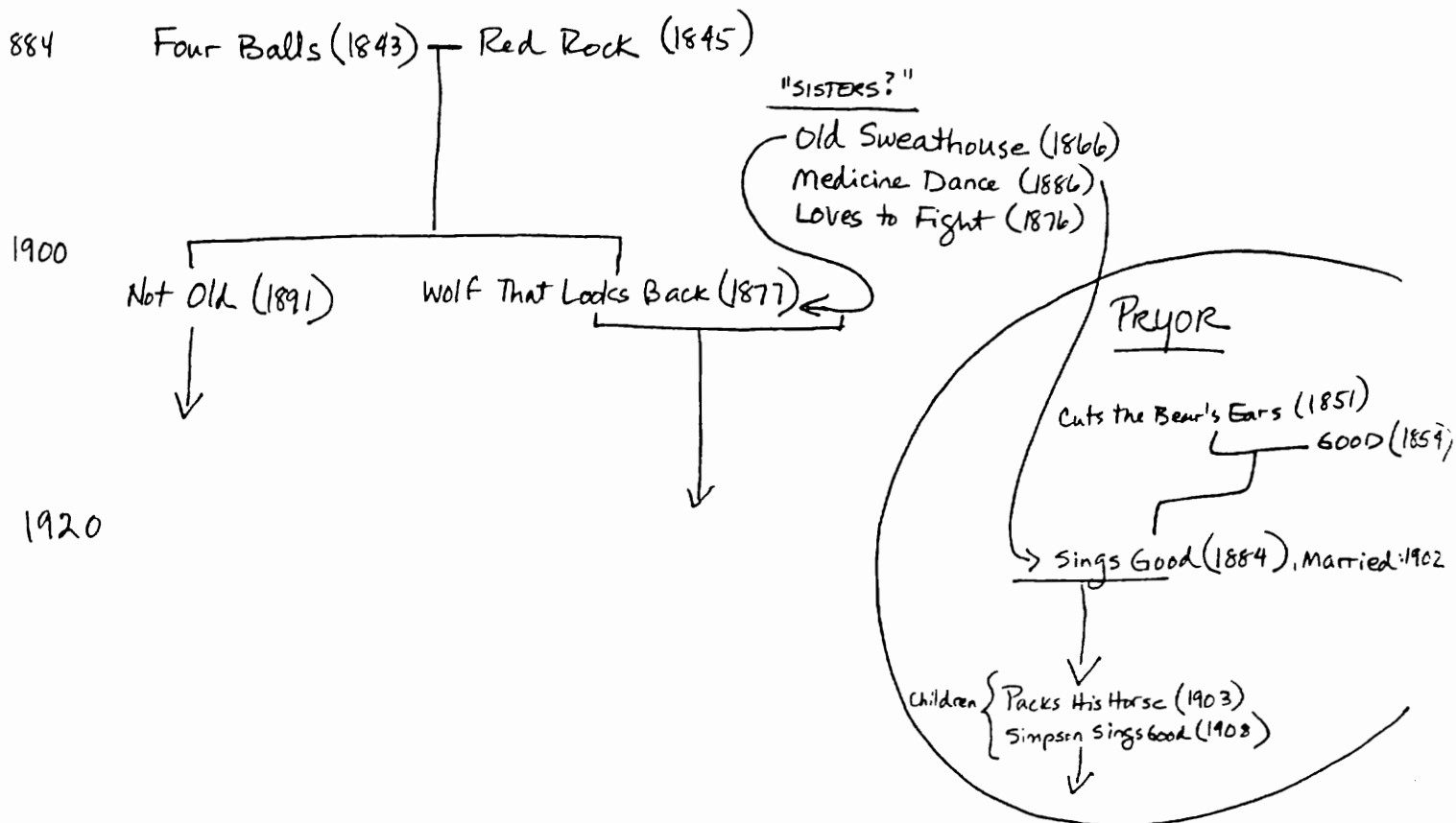


FIGURE 3: Plenty Coos First Families

Big Horn



Pryor

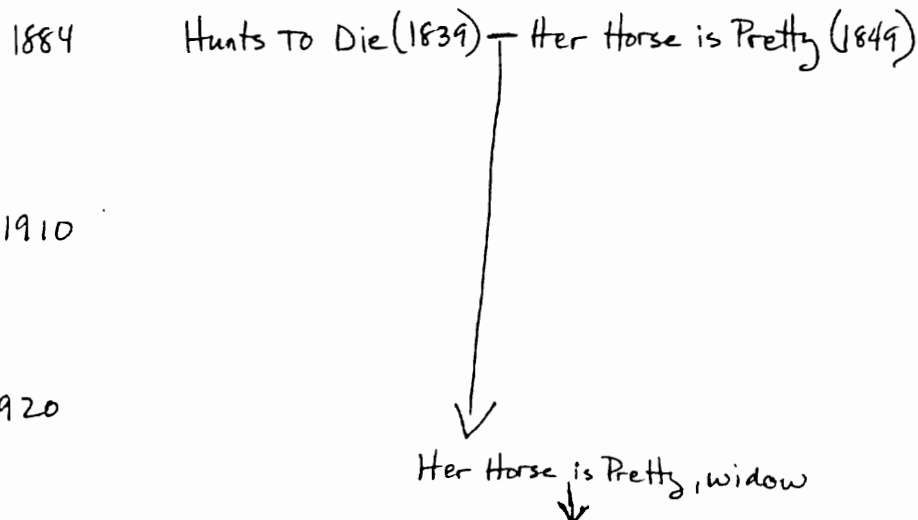
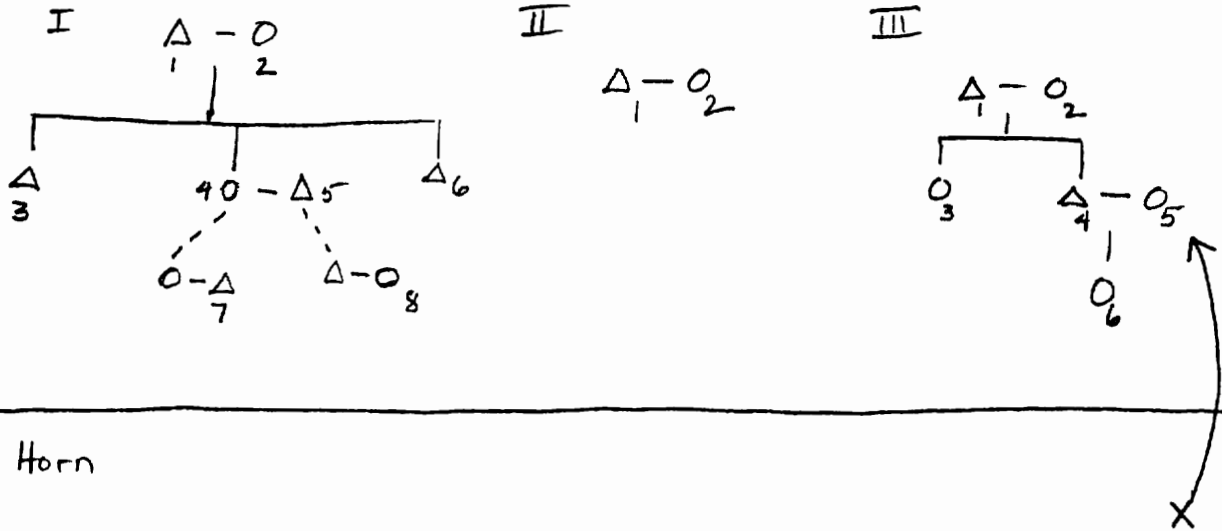
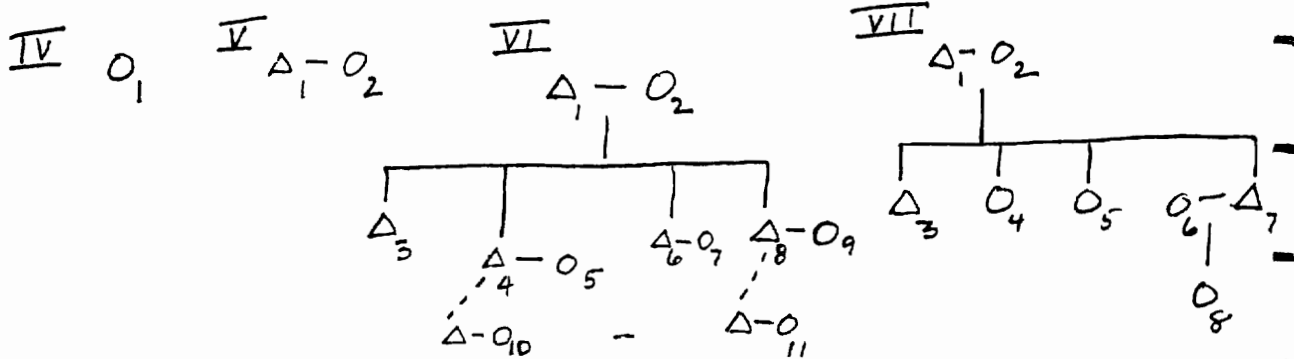


Figure 4: Nine Medicine Crow Families

Pryor



Big Horn



Little Big Horn



Figure 4: Names (birthdates in parentheses)

I.

- | | |
|----------------------------|--|
| 1. Snake Bull (1852) | 5. R. Spotted Arm (1880) |
| 2. Strikes The Iron (1852) | 6. B. Bull Snake (1904) |
| 3. Is Sweet Now (1895) | 7. P. Bird Hat (1887), second husband |
| 4. Plain Cedar (1890) | 8. Pretty Medicine (1877), second wife |

II.

1. Sharp Horn (1842)
2. Woman w/Eyes Open (1842)

III.

- | | |
|--------------------------|-----------------------------------|
| 1. The Crane (1857) | 4. End of Trail/Alex Crane (1886) |
| 2. Kills w/Horses (1860) | 5. Weasel High Up (1891?) |
| 3. "daughter" | 6. Gretchen Crane (1910) |

IV.

1. Little Wolf (1842)

V.

1. Skins A Wolf (1842)
2. Has Horses (1849)

VI.

- | | |
|---|--|
| 1. The Iron (1860) | 7. Good Hearted Ground (1875?) |
| 2. Pretty Medicine Rock (1862) | 8. Gets Down Well Known/Edward Iron (1887) |
| 3. Little Shield/Ernest Iron (1892) | 9. Magdalene Medicine Mane (1892) |
| 4. Woodpecker's Child/Robin Iron (1885) | 10. Emma Runs Between (1875), second wife |
| 5. Minnie Iron (1888) | 11. Angela Brass (1895), second wife |
| 6. Good Coos/Albert Iron (1895) | |

VII.

- | | |
|------------------------------------|---|
| 1. The Horn (1845) | 5. Florence Blaine (1895), granddaughter? |
| 2. All Alone (1855) | 6. James Blaine (1890), grandson? |
| 3. Goes After Spotted Horse (1884) | 7. Ethel Plentyhawk (1890) |
| 4. Mr. William Blaine (1874) | 8. Josephine Blaine (1909) |

VIII.

1. Medicine Crow (1851)
2. Medicine (1855)
3. Young Badger (1889)
4. Goes Pretty/Leo Medicine Crow (1894)
5. Chester Medicine Crow (1898)

IX.

1. Three Irons (1844)
2. Kills Many Men (1857)
3. Kills The Boy (1893)
4. Drinks All The Time/Victor Three Irons (1892)
5. John Wallace (?), #2's second husband

Table 1: CROW FIRST FAMILIES

By Band Number and District [135 who appear on allotment roll]

	<u>Band</u>	<u>Pryor</u>	<u>Big Horn</u>	<u>Little Big Horn</u>	<u>Total</u>
Adobe Town	1	0	0	0	0
Slim Belly	2	2	0	2	4
Bear in Water	3	1	0	4	5
Pretty Guts/ Long Otter	4	1	4	7	12
Fringe	5	1	1	1	3
Crazy Sister in Law	6	1	1	2	4
Old Dog	7	1	1	5	7
Medicine Crow	8	5	1	4	10
Iron Bull	9	1	3	2	6
Young Onion	10	0	2	4	6
Pretty Eagle	11	0	3	3	6
Bull Goes Hunting	12	0	0	1	1
Plenty Coos	13	8	2	2	12
Takes Wrinkle	14	1	3	4	8
Big Forehead	15	2	1	2	5
Long Elk	16	0	4	4	8
Horse Guard, (Sugar)	17	0	0	2	2
Bull Nose	18	0	0	4	4
Spaniard/Enemy Hunter	19	0	0	1	1
Two Belly	20	0	4	1	5
Old Crow	21	0	0	5	5
Spotted Horse	22	0	0	5	5
Crazy Head	23	0	1	0	1
Bear Wolf	24	0	1	4	5
Custer Scouts	25	0	0	0	0
Bear's Head/ Big Ox	26	0	0	2	2
Old Nest	27	2	2	4	8
TOTAL		26	34	75	135

Table 2 "Band" Populations, 1884

<u>Band</u>	<u>Families</u>	<u>Total</u>
1. Adobe Town	12	55
2. Slim Belly	20	111
3. Bear in Water	15	80
4. Pretty Guts/Long Otter	30	193
5. Fringe	19	101
6. Crazy Sister in Law	9	65
7. Old Dog	13	83
8. Medicine Crow	16	117
9. Iron Bull	20	135
10. Young Onion	26	152
11. Pretty Eagle	22	117
12. Bull Goes Hunting	10	80
13. Plenty Coos	22	146
14. Takes Wrinkle	28	202
15. Big Forehead	13	76
16. Long Elk	23	166
17. Horse Guard/Sugar	10	81
18. Bull Nose	17	116
19. Spaniard and Enemy Hunter	14	107
20. Two Belly	14	78
21. Old Crow	10	64
22. Spotted Horse	15	111
23. Crazy Head	0	0
24. Bear Wolf	13	87
25. Custer's Scouts	0	0
26. Bear's Head and Big Ox	10	56
27. Old Nest	14	103
Total	415	2682

Table 3: Division, Clan and Society Affiliations
of First Family Heads

<u>Band</u>	<u>Name</u>	<u>Division</u>	<u>Society</u>	<u>Clan</u>	<u>District</u>
Slim Belly (2)	Flathead Women	MT	Lumpwood	Big Lodge	Lodge Grass
Pretty Guts/ Long Otter (4)	Gros Ventre Horse	?	?	Greasy Mouth (adopted by Kicked in Bellies)	Lodge Grass
	Bull All The Time	?	?	Whistling Water	Lodge Grass
Fringe (5)	Hairy Moccasin	MT	Lumpwood	{ Bad Coups Kicked in Bellies Newly Made Lodge	?
	Child In Mouth	?	?		Pryor
	Push	?	?		Pryor
Crazy Sister in Law (6)	Goes Ahead	MT	Fox	Newly Made Lodge	Pryor
Old Dog (7)	Bread	MT		Whistle Water	Lodge Grass
	Old Dog	MT	Lumpwood	Sore Lip	Lodge Grass
	Arm Around the Neck	?	?	Newly Made Lodge	Lodge Grass
Medicine Crow (8)	Medicine Crow	MT	Lumpwood	Newly Made Lodge	Lodge Grass
	Horn	?	?	Newly Made Lodge	Lodge Grass
	Sharp Horn	?	?	Whistling Water	Pryor
Iron Bull (9)	Yellow Face	?	?	Tied in a Knot	Pryor
Young Onion (10)	Does Everything	MT	?	Filth Eating	Big Horn
	The Wolf	MT	Fox	Filth Eating	Lodge Grass
	Grey Bull	?	?	Filth Eating	?
	Yellow Crane	?	?	Sore Lip	Lodge Grass
Plenty Coos (13)	Hunts To Die	MT	Lumpwood	Sore Lip	Pryor
	Plenty Coos	MT	?	Sore Lip	Pryor
	Three Bears	?	?	Sore Lip	Pryor/Lodge Grass
	Bell Rock	?	?	Never Shoots	Pryor

Takes Wrinkle (14)	Sitting Elk Young Hairy Wolf	River River	Fox Fox	Never Shoots Never Shoots	Pryor Black Lodge
Big Forehead (15)	The Wet Big Snake	MT	Fox	Whistle Water Whistle Water	? Big Horn
Long Elk (16)	White Arm	?	?	Black Lodge or Lodge Grass	
Horse Guard or Sugar (17)	Big Ox (Sitting Elk, see #14)	River	Fox	Filth Eating	Black Lodge
Bull Nose (18)	White Man Runs Him	MT	Lumpwood	Big Lodge	
Two Belly (20)	Two Leggings	River	Lumpwood	Filth Eating	Black Lodge
Old Crow (21)	Wold Lies Down Bad Man	MT ?	Fox ?	Never Shoots Sore Lip	Big Horn Big Horn
Spotted Horse (22)	Spotted Horse Flat Dog Shows A Fish Grandmother's Knife	? ? MT ?	? ? Fox ?	Piegan Lodge Piegan Lodge { Big Lodge Newly Made	Lodge Grass Lodge Grass Lodge Grass Lodge Grass
Bear Wolf (24)	Pretty on Top	?	?	Big Lodge	Lodge Grass
Bear's Head/ Big Ox (26)	Young Hairy Wolf (see 14)				
Old Nest (27)	Two Whistles Good Luck	MT ?	Lumpwood ?	{ Filth Eating Tied in Knot	Lodge Grass Lodge Grass

Clan Names:
 { Newly Made Lodges
 Thick Lodge or Big Lodge
 { Sore Lip or Burned Mouth
 Greasy inside the Mouth
 { Kicked in the Bellies
 Bad War Honors or Bad Coups
 714-56-10

{ Without Shooting They Bring Game or Never
 Shoot, Fetch Game
 { Tied in a Knot
 Filth Eating Lodge
 { Whistling Water
 Streaked Lodge
 { Piegan Lodge
 Treacherous Lodge

{ = Affiliated

TABLE 4
AGE COHORTS, 1900

Age Group	Men	Women
70+	19	42
60-69	43	53
50-59	129	146
40-49	125	119
30-39	174	148
20-29	137	126
10-19	117	106
0-9	191	202
Total	935 men	940 women

TOTAL: 1875

714-56-07

TABLE 5

AGE COHORTS, 1910

27	70+	35	
63	60-69	92	
90	50-59	83	
141	40-49	116	
131	30-39	116	
98	20-29	91	
170	10-19	159	
202	0-9	210	
922 men		910 women	

TOTAL: 1832

714-56-07

CROW POPULATION CHARACTERISTICS

Table 6

Blood Quantum, 1900-1910

	1900		1910	
% of Indian Blood	n.	% of total	n.	% of total
100	1698	.91	1448	.77
75	21	.01	73	.04
50	85	.05	209	.11
25	32	.02	73	.04
0	27	.01	73	.04
TOTAL	1863		1876	

Table 7

Tribal Affiliation, 1900-1910

	1900		1910	
Tribe	n.	% of total	n.	% of total
Crow	1768	.96	1784	.98
Other tribes	71	.04	34	.02
Total	1839		1818	

Table 8

Language Spoken, Crows Over 18

	1900		1910	
	n.	% of total	n.	% of total
"Crow, No English"	1065	.82	754	.66
"English"	217	.17	382	.33
No answer	15	.01	15	.01
Total	1297		1151	

CROW MARRIAGE PATTERNS

Table 9 Marital Status, Crows 18 and Older

	1900		1910	
	n.	% of total	n.	% of total
Single	77	.06	103	.09
Married	990	.76	875	.76
Widow	227	.18	161	.14
No record	2	--	4	--
Total	1296		1151	

Table 10 Number of Individuals Married, by Birth Cohort, 1900

Age	0-19	20-29	30-39	40-49	50-59	60-69	70+
Men	4	91	153	106	108	32	11
Women	22	120	137	99	96	21	5
Total	26	211	290	205	204	53	16

Table 11 Number of Individuals Married as Percentage of Total Birth Cohort, 1900

Age	0-19	20-29	30-39	40-49	50-59	60-69	70+
Men	.01	.66	.88	.85	.84	.74	.58
Women	.07	.95	.93	.83	.66	.40	.12
Total	.04	.80	.90	.84	.74	.55	.26

Table 12 Mean Lengths of Present Marriage, By Birth Cohort, 1900

Age	0-19	20-29	30-39	40-49	50-59	60-69	70+
Men	2	5	11	20	28	35	41
Women	2	7	13	22	30	31	54

FREQUENT MARRIAGE TABLES, 1910

Table 13 Frequency of Marriage, 1910

No. of Times Married	1	2	3	4	5	6	7+
No. of Individuals	292	249	116	70	30	24	48
Percentage of Total	.35	.30	.14	.08	.04	.03	.06

Table 14 Length of Present Marriage, Crow Men Married More than Five Times, 1910

Age Cohort	30-39	40-49	50-59	60-69	70+
No. of Individuals	1	19	14	15	9
Mean no. of marriages	7	7	7	8	10
Mean length of present marriage (no widows)	7	16	22	26	33
Range of marriage lengths	7	1-33	5-36	1-45	14-50

Table 15 Length of Present Marriage, Crow Women Married More Than Five Times, 1910

Age Cohort	40-49	50-59	60-69	70+
No. of Individuals	5	3	9	6
Mean no. of marriages	7	6	7	8
Mean length of present marriage (no widows)	9	22	27	39
Range of marriage lengths	5-15	7-30	12-35	27-50

Comments by
Caroline Brettel

on

SEARCHING FOR STRUCTURE

Linking Manuscript Census Returns to Explore the Social Structure of
the Early Twentieth Century Crow Reservation

While I think my major task here is to comment on the methodological promise and problems of Hoxie's attempt to link individuals across several different censuses, since he begins his paper with a discussion of how this methodology may shed new light on questions of cultural persistence among American Indian groups, I cannot resist bringing to your attention--in case you missed it--a recent FAR SIDE cartoon in which a group of "natives" consult their compendium of 101 raindances (we are not sure of its authorship) before teaching an anxious anthropologist about their traditional rituals. So goes the common "joke-on-us" these days in anthropological circles--the informant, in the course of an ethnographic interview, stops the interviewer for a moment while he pulls out his Evans-Pritchard if he is a Nuer, his Malinowski if he is a Trobriand Islander, or his Robert Lowie if he is a Crow Indian. All kinds of anthropological-angst is embedded in this joke, but clearly the issue of cultural preservation (and who is doing the preserving) is central. Another of my favorite FAR SIDES (clearly the cartoonist Larson must like to poke fun at anthropologists) shows several natives running to hide their television sets and VCR's when they see safari-hatted anthropologists walking up the hillside toward them.

Beyond the statements that these cartoons make about the issue of cultural persistence, we can perhaps also draw, at least in the case of the first one that I described, a message about the sources we use and who created them. Ethnohistorians generally take the contact scenario as a premise for evaluating qualitative sources that shed light on Indian history. The same rules should apply to quantitative, or quantifiable sources. The census lists that Hoxie is dealing with were made by outsiders, and we therefore have to ask ourselves constantly as we use them the extent to which they represent the social structure of insiders or social groupings created by outsiders.

Before I go on with this point, one thing should be said right upfront, so that historians of Indian culture do not retread the ground over which historians of the American and European family have passed. Qualitative does not necessarily mean non-concrete and quantitative is certainly not nirvana, although some funding agencies would have us believe so. The synthesis in, for want of a better word, "mainstream" family history is one that looks at both mentalite or attitudes and behavior. To my mind, quantitative data should serve as a complement rather than an alternative. Thus, Lowie's reconstruction of pre-reservation Crow culture should be played off against the conclusions about social structure that can be drawn by analyzing the 1884 listing discussed by Hoxie.

At this point, an aside is in order. It seems to me that historians of Indian culture and society will have to be as aware as their brothers and sisters in other branches of social history of the

difficulties of inferring attitudes from behavior. In an important note in Historical Methods a few years back, Adrian Wilson proposed a simple methodological constraint to be placed upon inferential attempts--that an inference about attitudes made from evidence about behavior is acceptable only if it implies predictions about behavior that can themselves be tested. Hoxie does not move this far in his preliminary analysis, but as he develops his theories about attitudes toward marriage, for example, he may want to keep this caution in mind. How do we get from who married and when they married, for example, to attitudes and beliefs about marriage and family formation?

But, I am jumping ahead of myself, so let me return to the issue of insider versus outsider categories. Beginning in 1887, according to Hoxie, we have an annual enumeration of individuals by "family" (he cautiously puts it in quotation marks). What kinds of families are these? Are they coresidential units or kin groups? What we know about the way that this enumeration was made should be specified so that there is no doubt. Certainly "family" in 1887 was not at all the definition of "family" that Hoxie attributes to George W. Frost, the Agent to the Crows a decade earlier, who claimed there were no families because--to paraphrase--marital rights are disregarded, polygamy and adultery are common. Frost carried his own definition of "family" in his head and clearly it was not a Crow definition if we are to begin from the premise that polygamy or several serial marriages are PART of the structure rather than an evidence of an absence of structure. The long-term retrospective picture that Hoxie develops certainly does shed light on the "stability" of final unions,

although the series of short-term initial unions is clearly a social feature that requires more explanation.

While I do not fully understand the way in which the pre-1900 enumerations were taken and what "family" really meant, the 1900 and 1910 manuscripts were clearly constructed according to the overall guidelines for the US census in each of those years. These are residential units--collections of kin and non-kin. While Hoxie is more interested in linkages (something that I will come back to shortly), I think it would be very useful to analyze each of these manuscripts cross-sectionally to give us some indication of household structure at two or more points in time--what are the varying types of residential collectivities? Are we always dealing exclusively with kin-groups or are non-relatives present? If so, who are they? If a "family" in 1884 was made up of a male head of household, two other men, four women, and six children and the same head of household is found in 1900 with his wife and two children, what are we looking at? Does the 1900 manuscript entry represent the reservation pattern of settlement imposed on the Crow; does it represent a necessary adaptation from hunting and gathering to settled agriculture; or is it a natural result of the developmental cycle? These are all possibilities to be explored.

I raise the concept of developmental or family cycle. In fact, from the preliminary data presented by Hoxie, it seems evident that for Crow Indians, just as in regions of the Caribbean, Pacific Islands, and Africa, the western family cycle model does not apply. In this

regard, I would suggest that Hoxie consider the alternative life-course model that has been forwarded by scholars of non-western societies (and use in the western context subsequently) as a way to deal with changes in household composition through time. Such a model looks at factors accounting for the changing coresidential arrangements of individuals through the life course--what, we might ask, are the social, economic, and demographic factors that influence coresidential choices; do these change over time; and what are the implications of specific coresidential situations for subsequent life experiences? While the individual graphic descriptions included in Hoxie's paper are fascinating and suggestive, (though the tribe's social structures are hard to decipher) a method based on the individual life course will facilitate both analysis and comparison. You could, for example, and assuming that you have ages of individuals available at varying points in time, look at the varying household arrangements (close study of the data would give you the relevant arrangements) in which one finds single men between 15 and 19 in different years. What then, about men of 20-24, 25-29, etc. etc.?

Clearly, the data used by Hoxie to study social structure and persistence can also be used to study the changing demography of the Crow during the reservation period. He gives us powerful data on mortality--16% of children under ten died between 1900 and 1910. In his concluding remarks, he notes the possibility of studying when people first married. Post-marital residence patterns could also be examined. With regard to fertility, child-woman ratios and birth intervals can be calculated. In fact, with such a high degree of

persistence, and with the successive lists that Hoxie has at his disposal (many of which remain uncoded), the entire fertility history of women can be studied--not just how many children, but how many survived, and for what length of her reproductive cycle did a woman continue to bear children. What was the impact of serial marriage on childbearing?

Any sort of record linkage must be carried out with specific research questions in mind. Record linkage is not an end in and of itself--although the fact that high proportions of Crow Indians stayed on the reservation for most of their lives makes record linkage in this context particularly satisfying. If this phenomenon is generalizable to other reservation Indian tribes, the data available to study changing/persisting social structure through time will be remarkable. Hoxie asks a valid question about cultural persistence, suggests two social structural measures of persistence--residence and marriage--and then proceeds to examine these measures. The approach is solid. The assumption is that if a social structure survives, a culture survives. It is a reasonable assumption. However, given the complexity of this social structure, I would be careful to sort out the units of analysis--band, clan, family or kin group, household. In what contexts are each of these important. When he finds an individual moving from one region of the reservation to another, are there any conclusions that can be drawn and then tested about possible reasons for this move--a band connection, a kind connection? Can we find an explanation for why some members of a band settled away from where the majority of band members settled--was there some

band-exogamous kinship link that we can track down? I am not sure that any of the data at Hoxie's disposal can adequately answer the question, but it is at least worth a try.

We were asked in our comments to address the question of whether the papers advance in some way our understanding of American social history. Clearly, studies such as this one of persisting Indian social structure in the context of reservation life do provide further evidence of the fact that the family-cycle model is western-oriented and carries little weight in a cross-cultural context. The band as an important level of social organization that influences marriage decisions is a concept foreign to "mainstream" family history but something to think about. It is significant to learn that these Indians persisted in band endogamy but developed (if that is the right word) district endogamy. What we learn about shifting conjugal relationships is equally important. What one wants to learn, should Hoxie's method and types of data be explored among other Indian groups, is the degree to which this was a pan-Indian phenomenon. Should we be able to carry the analysis forward several decades (with sources other than the US manuscript census since post 1910 manuscripts will not be available to study for many years), it will be interesting to know what happens to these marriage patterns? Do those Indians who grow up primarily during the reservation period have fewer unions than their parents had? And then, what does this tell us about the meaning and impact of the shift from hunting and gathering to settled living?

In conclusion, the sources that Hoxie describes and analyzes, particularly if they are ultimately used in conjunction with the range of other sources that he mentions in his conclusion, can only enrich the study of American Indian culture and society. They need to be used cautiously--a fact that Hoxie is well aware of--but there is enormous potential here. Hopefully the same excitement that was generated among family historians more than two decades ago when the French demographer Louis Henry first introduced his method of family reconstitution will emerge to spur on many of you who are gathered here today. Is so, we may know as much some day about the changing social and family structure of American Indian groups in the late 19th and 20th centuries as we know about the marital choices and residence patterns of English cottagers, Italian sharecroppers, and Portuguese peasants.

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POLITICAL POWER AND POLITICAL ORGANIZATION:
THE QUANTITATIVE ASPECT OF COMANCHE POLITICS,
1786 - 1875.

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Political Power and Political Organization:
The Quantitative Aspect of Comanche Politics,
1786 - 1875.

Thomas W. Kavanagh

Abstract

This paper examines the quantitative aspect of Comanche political organization, 1786 - 1875. Comanche organization during this period has been the subject of continuing confusion in the historical and anthropological literature. A variety of solutions have been proposed; however, most do not address the underlying quantitative aspects of political organization.

The relation between political power and political organization is simply this: variation in political resources implies variation in the political organization derived from those resources. Stated another way, other things being equal, a political organization can last only so long as the resources upon which it is based. Therefore, attention to variations in the spatial, temporal, and political-hierarchical dimensions of Comanche political resources should illuminate the variations in Comanche political organization.

Comanche political organization was based on the control of four types of resources: (1) those based on the buffalo and the horse; (2) those based on the redistribution of war booty; (3) those based the control of trade; (4) those based on the possession and redistribution of Euro-American political gifts. Variations in these resources can be related to variations in the organization of Comanche operating units.

In the eighteenth century, the two major divisional organizations, the Kotsoteka and Yamparika, each initially based on

control of the buffalo and horse and from the redistribution of war booty, were encapsulated within the Spanish sphere of influence. While the proximity of the Yamparika to Anglo-American traders on the Arkansas allowed their organization to survive the collapse of the Spanish empire and the loss of political support, the Kotsoteka organization did not. While a remnant Kotsoteka organization continued until the reservation, two new groups emerged, the Penateka of southern Texas, dependant upon Anglo-American political support, and the Quahada of the Staked Plains, whose political and economic power was derived from the trade in stolen cattle. On the north east along the Red River, two other groups, the Tenewa and the Nokoni, were based on the horse trade with Spanish of Natchitoches and the Anglo-Americans of Natchez and St. Louis. The divisional organizations finally collapsed on the reservation, with the Agent's refusal to recognize the position of "Principal Chief."

I

Politics has been defined as "who gets what, when, and how" (Lasswell 1958). This aphorism, while simplistic, points out the importance of quantitative data in the analysis of political systems; attention to names, numbers, and the relations between them is a necessary part of historical research. As historian Robert Berkhofer noted some time ago, to answer even the most basic questions of Indian political organization, we must know "who were the chiefs and what was the nature of [their] support" (1971:375). Unfortunately, relatively few studies of American Indian political systems take these details into account. [1] As an example of the usefulness of this kind of data, this paper examines the quantitative aspect of Comanche political organization between 1785 - 1875.

Comanche political organization has been the subject of continuing confusion in the historic and anthropological literature. One of the most common activities in Comanche studies is the game "Trace the Band," the attempt to un-tangle the web of names, translations, and political relationships between various named Comanche social groups. The primary goal in "Trace the Band" is to explain the variation and gradual increase in the number of named groups through time. Several general solutions have been offered for the problem: that "only the larger bands received permanent recognition and a name"; that groups might have multiple names; that there was a gradual increase in European knowledge of the Comanche; that the date of first documentary mention corresponds to a chronology of migration (Wallace and Hoebel 1952:25ff; Hyde 1959:58; Schlesier 1972; Thurman 1981:7).

While a degree of validity may be granted for each of these explanations, the whole is not particularly elegant. They are a set of particularistic solutions, and as a whole they do not address the processual nature of political organization.

II

Let me begin by laying out a framework for analysis. I am interested in political organization, with the "working arrangements of society," (Firth 1954:10), the ordering and re-ordering of society through time and across space. More particularly, I am concerned with the actual operating units of Comanche political organization, the bands and divisions, and the processes of change operating on them.

Organization can be influenced by a variety of factors. On the one hand is the normative cultural structure, the shared understandings about the world and how to get things done in it (Swartz and Jordan 1980; Bailey 1969). But while culture provides "guides" for organization, it is not determinative of organization. The translation of cultural models into organization is mediated by the situational exigencies of the moment. As Firth noted, "studies in social organization demand attention to three criteria: the magnitude of the situation (as in men and materials); the alternatives open for choice and decision; and the time dimension" (1954:12).

I am especially concerned with the sociological dimensions of political power. Political power is, in general, the ability to influence the behavior of others through the control of access to strategic resources. I must emphasize that political resour-

ces can be both material as well as non-material. On the Plains, the non-material resources, puha, medicine, wisdom, or age, were as much strategic resources as were control of traders goods. [2] But whatever the existential nature of resources, the control of resources results in a "power domain," the sphere of influence generated by control of a particular resource (Adams 1966). While a power domain may be abstract or an unorganized class of people concerned with a particular resource, it can also be a concrete social unit. [3]

The relation between political power and political organization is simply this: variation in the resources upon which a power domain is built implies variation in both that domain and all other domains derived from it. Stated another way, other things being equal, a political organization can last only so long as the resources upon which it is based. [4] Therefore, attention to variations in the spatial, temporal, and political-hierarchical dimensions of Comanche power resources and domains should illuminate the variations in Comanche political organization.

III

The first step in the resolution of "Trace the Band" is a clarification of Comanche political integration. Four levels of integration can be recognized, the nuclear family, the extended family or nemenakane, the local band, and the regional division (Kavanagh 1986). Local bands were composed of two or more nemenakanes, plus attached but unrelated families and individuals. The leader of the core nemenakane was the local band headman. While

it may be assumed that local bands were names, and that many of the names contained on the band lists refer to local bands, because of the fluidity of local bands, it is not always possible to assign ethnonyms to particular local bands.

The maximal level of Comanche political organization were the divisions, groups of local bands linked by kinship and sodality ties. Divisional principal chiefs were elected from among the ranks of the component local band chiefs.

While this was the normative, structural model of Comanche socio-political integration, the actual operating units of Comanche society were dynamic; their continued existence depended upon their leaders' ability to attract and maintain a following through the control of resources.

Comanche power domains were based on the control of four types of resources:

- (1) the buffalo and the horse.
- (2) the redistribution of war booty.
- (3) the control of trade.
- (4) the possession and redistribution of Euro-American political gifts.

(1) The Buffalo and the Horse

There is little information on the direct role of the buffalo and horse in Comanche political organization. Still, several general observations can be made. In pre-horse times, the primary hunting technology on the Plains was the "drive," in which animals are lured into a trap or stampeded over a cliff. Often called "communal," these hunts were under the general control of

a "pound-maker," often a shaman, who controlled the distribution of the product of the hunt. To be sure, this distribution may have been hedged with normative imperatives towards sharing, but the pound-maker's central position and power domain cannot be denied.

The introduction of the horse would have increased a group's hunting efficiency by expanding the range of search parties and easing the work of maneuvering the herd to the trap. At the same time, by controlling access to this expanded range, a horse owner could create a power domain encompassing horse borrowers, in many cases including the whole community. But increasing horse populations would have decreased the importance of such domains by decreasing both the value of individual horses and of the necessity for co-ordinated group hunting. In post - horse times, and particularly for the horse rich Comanche, while the buffalo and the horse maintained an importance in the domestic economy and in the cosmology, their political importance was as trade items. [5]

2) Redistribution of War Booty.

The spoils of war constituted a power domain under the control of the leader of the particular war party. As Hoebel noted, "a reputation for fairness in dividing the spoils enhanced the prestige of a war leader and gained him followers" (Hoebel 1940:25).

Unfortunately, at present, the quantitative dimensions of such domains are unclear. Few Comanche war accounts contain numbers, being more concerned with the prestige and war honors earned [6] and while European booty played a major role in Coman-

the political economy, Europeans often exaggerated its extent. In 1850, it was estimated that Comanches had done \$1 million in damages to northern Mexico (Emory 1857). In 1873, John Hittson said that in the previous twenty years, 100,000 Texas cattle were lost to the Indians (Kenner 1969:174). Charles Goodnight claimed the figure was 300,000 cattle and 100,000 horses (Haley 1935:169).

Although these figures have been accepted by many scholars, they must be approached with caution. Examination of the actual claims gives a less inflated estimate. Tables 1.1 - 1.2 shows claims made by citizens of three counties in the pre-Civil War period. In the period 1847 - 1861, citizens of Refugio county claimed losses of \$18,107 in horses and cattle, an average of \$1,646 per year. In a similar period, 1849 - 1861, Nueces county claimed 506 horses and 253 cattle were taken, an average of 42 horses and 21 cattle per year. In the years 1857 - 1861, Montague County lost 135 horses and 18 cattle, an average of 33 horses and 4.5 cattle per year (Kavanagh 1986:237, Appendices 17:1 - 17:5).

In the post - Civil War period, the Texas Adjutant General's Office made several surveys of county officials for details on the stock stolen, one in 1867, another in 1873, a third in 1875:

1865-1867		Prior to 1873		1873 to 1875	
Horses	Cattle	Horses	Cattle	Horses	Cattle
4,479	32,884	6,198	8,685	3,809	1,160

If there is no duplication in these figures, in ten years, 14,483 horses, and 42,729 cattle were claimed as stolen, an average of

145 horses and 427 cattle per year (see Table 1.2; Kavanagh 1986:238, Appendix 17:6). It must be remembered that not all these animals were taken by Comanches; however the figures give an estimate of the potential of such power domains.

3) The Control of Trade

There were three general Comanche - European trade contexts: 1) trade fairs, both formal and informal; 2) "Comanchero" and other mobile traders; and 3) trading posts.

Comanches had participated in trade fairs in New Mexico since the first contacts. Taos and Picuris pueblos were early centers for these fairs, but gradually locations in the Pecos valley, Pecos pueblo itself, San Miguel del Bado, Anton Chico, and elsewhere, became informal ports of entry between New Mexico and Comancheria. Although often banned, there was constant commerce between the Comanche and New Mexico settlements.

That Texas Comanches conducted trade during their visits to Bexar (San Antonio) is undoubted. There are, however, no reports of formal trade fairs as in New Mexico. Later, Comanches often traded with the Germans at Fredericksburg, but there is, as of yet, little information on the extent of this trade.

By the late 1780's, trade had begun to be carried to the Comanche camps by licensed traders. In New Mexico, these traders were first known as "viageros" (John 1984:346); by 1813, the word "Comancheros" was being applied to them. (Levine and Freeman 1982:6). [7] While some Comancheros relied upon chance meetings with Comanches (Haley 1935:163), others went to specified rendezvous and more or less permanent trading stations. [8]

On the east and north, Spanish traders from Nacogdoches and Natchitoches, both legal and contraband, followed the "Comanchero" pattern. But again, there is little information on these easterners, and only a few names are known. Similarly, although Anglo - American traders began visiting Comanche camps as early as 1795, relatively little is known about them. [9] In seasons of peace during the Republic, many Texans visited Comanche camps (Richmond, Texas Telescope, 27 April 1838).

The other trading context was the more or less permanent post. Seldom attempted by Spaniards or French, [10] the first American post in Comancheria seems to have been Thomas James' 1823 "fort" on the North Canadian (James 1984:127). By the 1830's, Comancheria was ringed with posts, the various Bents Forts on the Arkansas, several posts on the Red River, and others in central Oklahoma. [11]

Comanches traded the animal products of the Plains, meat, tallow, hides, as well as the animals themselves. At the 1786 Pecos Fair, the Comanches traded "more than six hundred hides, many loads of meat and tallow, fifteen riding beasts, and three guns to their entire satisfaction" (Thomas 1932:306). The Comanches who visited Warren's post on the Red traded "furs of all kinds, dressed buffalo robes, dressed and raw deer skins, dried buffalo tongues, beeswax" (Cliff 1924:139).

There are few accounts of the Comanche share of the later fur and hide trade. In 1856, it was estimated that the Comanches had an annual income of \$10,000, based on the consumption and sale of the meat and hides of 30,000 buffalo, 2,000 deer, 1,000

elk, and 500 bears (NA M234 R878-103). Eleven years later, a Saint Louis trader returned from Comancheria with 1,699 buffalo robes and a "large amount of peltry" (NA M234 R375-660).

As opposed to the fur trade on the Northern Plains, the horse trade was the primary context for early Anglo-American presence in Comancheria, and the trade with Americans stimulated other Indians to visit Comancheria as middlemen. In 1848, visiting Osages purchased "1500 head of mules worth \$50-75,000" (NA M234 R858-249) which were then were passed on to Anglo-American buyers in St. Louis. It should be noted that this trade was not only in horses bred by the Comanche, but included horses stolen in Texas or Mexico. That is, they were war booty transformed into commercial commodities.

By the 1860's, another animal product joined the Comanche trade: cattle. As with the horse trade, this was stimulated by a market, this time in New Mexico. It was later claimed that several prominent New Mexicans, including the post sutler at Fort Bascom, established their herds with these Comanche cattle (Kenner 1962). [12]

In the Comanchero mode of trade, the chiefs controlled the entire context of trading. The trader's goods were deposited in the chief's lodge for "safekeeping," and only when the chiefs had received pre-trade gifts were the people allowed to trade. Conversely, since as each camp was under a specific chief, as were the major trading stations, the ability of the chiefs to ensure a degree of order and security greatly increased the willingness of traders to venture across the Plains. [13]

There was less chiefly control of trade at the posts. Traders attempted to impose control by permitting only a few Indians inside the walls at a time, or by cutting "ticket windows" through the walls. But still, trade did not start until the chiefs approved it. [14]

4) Political Gifts

The most important Comanche power domains were created by the redistribution of European political gifts. The various European nations with whom the Comanches dealt all used political gifts and Comanches were well aware of the costs and benefits of these gifts. [15] In 1801, when war threatened in Texas, the New Mexico Comanches stepped in to end the dispute, saying that it would result in the "loss of many benefits" both in Texas and New Mexico (BA 30:510).

Great amounts of money were directed towards the Comanche. Between 1789 and 1820, New Mexico claimed expense of almost 74,000 pesos for gifts to the "Allied Nations," including the Comanche (Table 2.1). Table 2.2 shows the Comanche share (48%) of the total amounts distributed from Santa Fe in the years for which both figures are available.

The funds from Béxar are less well documented. Annual summaries giving amounts received, expended, and year-end balances are rare, as are monthly or quarterly account memoranda. Table 3.1 is a preliminary account of the Béxar funds, 1787-1810, showing some 24,000 pesos (one-third the amount available in New Mexico) distributed to the Indians of Texas. The most complete year recorded in Béxar is 1810 (BA 47-420). That year, 3,905.6

pesos were spent on 2,425 visitors, of whom 1,374 (56%) were Comanche (see Table 3.3). But some years, such as 1819, no Comanches visited Béxar, or at least none were recorded.

In the years after 1821, the Mexican Republic attempted to maintain the good relations with the Comanche, but in both New Mexico and Texas, Mexican-Comanche relations were constantly hampered by a lack of cash. In New Mexico in 1821, because of the lack of public funds, officials were forced to hold a private subscription for Comanche gifts. Several diplomatic meetings with the Comanche were threatened because of the lack of gifts (SANM T-3010). Consequently, compared to earlier times, relatively few Comanches visited Mexican Santa Fe peacefully.

A similar situation existed in Mexican Texas. In 1822, the Mexican government appropriated 2,000 pesos for Texas Indians (BA 70-87). But this money did not arrive until 1825. In 1834, there was no money at all for gifts (BA 164-86). In May, 1835, an attempt was made to re-establish Comanche gifts (BA 165-38), but action was cut off by the Anglo-American Texas Revolution.

Comanches received political gifts from the Texas Republic (Smithwick 1983:136). Sam Houston's 1844 treaty included the provision that presents would be given "as the President from time to time shall deem proper" (Article XIV). But the Texas Republic Indian accounts are incomplete, and auditing difficult. [16] In 1861, the Confederates promised some \$64,862 in goods for the Comanche (Abel 1915:322), but there is no record of whether the money was appropriated, or if delivered, how it was spent.

Under the United States regime, money for Indian gifts was generally dependent on the existence of treaty relations (no

treaty - no money), and the money was generally viewed as part of a land transaction, rather than as political support. There were, however, some exceptions to the treaty requirement. In 1805, John Sibley had a budget of \$3,000 for Comanche presents "to be distributed in the most useful manner" (Carter 1954; 20:352). In the late 1840's, both in Texas and along the Arkansas, gifts were distributed apparently without treaty authority. From all the treaties, some \$390,000 or more was to have been spent on the Comanches and their allies between 1835 and 1870 (see Table 4).

But while Table 4 shows the amounts specified, as with the Texas Republic accounts, the actual amounts of the late pre-reservation period are confused, and it is not always clear how the money was actually spent, or if it was spent at all. Furthermore, treaty monies had to be appropriated by Congress, and Congress was not always willing to act. There were several "withholdings" of annuities, particularly of the Fort Atkinson treaty goods in the 1850's, and of the Medicine Lodge goods in the late 1860's, due to accusations of raiding, etc.

Political gifts supported the Comanche political organization in two ways. First, there was the distribution of subsistence goods. In the Spanish period, Comanche visitors to the Spanish capitals were fed at a rate of 2 reales per day (SANM T-1400) to cover the cost of provisions during their stay. They were also "regaled" with metal goods, knives, copper kettles, and horse tack, as well as beads, cloth, trimming, etc. [17] Similar American goods ran the spectrum of American manufactures. [18] But although individual Comanches might show up at European

settlements to obtain goods, most Comanches obtained access to European goods through the good offices of their chiefs. [19]

At the same time, elite, prestige gifts legitimized Comanche leaders. [20] As with much of Spanish America, leaders were recognized with silver headed canes of office. [21] Two sizes of medals were given, large ones for principal chiefs and "Generals," smaller ones for capitancillos or local band chiefs (BA 17-97). [22] Flags had long been used as symbols of alliance; French, Spanish, or Mexican flags were common sights in Comanche villages. [23] Another major prestige gift was cloth and clothing. [24] For instance, when Francisco Amangual visited Ysapaupa's camp in May of 1808,

the big chief and the other chiefs of the tribe came out to meet us. They were very well dressed, but they wore very unusual clothes: long red coats with blue collars and cuffs, white buttons, yellow (imitation gold) galloons; one was dressed in ancient Spanish style: a short red coat, blue trousers, white stockings, English spurs, an ordinary cornered hat worn cocked, a cane with a silver handle shaped like a hyssop. Others wore red neckties; they wore sashes made out of otter skin, adorned with beads and shells. (Loomis and Nasatir 1967:482).

Probably the most important prestige gifts were firearms. Most firearms were channeled through Béxar as there are few references in the Santa Fe invoices to firearms, powder, ball, etc. [25]

Anglo-Americans prestige gifts included "Peace Medals," flags, swords, and uniforms. [26] On the Texas reservation, Ketumsee was paid thirty dollars per month "so he could devote his time to his duties and not have to hunt" (Neighbors 1975:182), and he was the only Indian with a permanent house on the reservation. On the post - Civil War Fort Sill Reservation, chiefs were promised houses, but it took almost seven years for

them to be built. Several were given army ambulances carriages "for services to the government and to strengthen [their] influence" (OHSKA 47-97). Firearms and ammunition and ammunition was also given, but there is little information about the numbers and calibers involved. [27]

IV

The Organization of Comanche Power Domains

Although much of this data is incomplete, a beginning can be made towards an analysis of the organization of Comanche power domains. First it may be noted that there is a hierarchical dimension to Comanche power domains. As both food and as item of trade, the buffalo supported single families and *nemenakane* (extended families). Leaders of war expeditions controlled the distribution of war booty. Local band chiefs controlled trade. Both Principal Chiefs and local band chiefs had control of the distribution of subsistence gifts. Finally, prestige gifts went to Principal Chiefs and "Generals."

There are also temporal and spatial - geographic dimensions to the organization of Comanche power domains. In pre-horse times, through control of the communal hunting technology, the pound-maker controlled access to the buffalo. Initially, the horse merely enhanced this domain. Horse owners and their social groups, the men's societies, through their control of the hunt enabled the continuance of this domain. But as horse herds increased, access to both the buffalo and later to European goods which could be obtained through the horse trade was "democratized."

This general Plains-wide developmental trajectory forms a backdrop to the specifics of Comanche political organization, and the effects of their relations with Europeans. The initial Comanche move south was probably a desire to obtain horses (Richardson 1933:19). This move also brought direct contact with European goods, and control of access to these goods soon became the most important basis of political power. While warfare and raiding was a possible source of supply, the ability of a leader to get along with the Europeans increased his, and his people's, access to these goods. But because the Comanche divisions had differential relations with the different Europeans, there were different resulting organizations and trajectories of change.

The Kotsotekas, Penatekas, and Quahadas

The Kotsotekas ("Buffalo Eaters") were the first Comanches to have extensive contact with the Spaniards of New Mexico. When they first appear in the Spanish records, the Kotsoteka had a developed political organization headed by an elected Principal Chief wielding considerable political power. This power was probably derived from control of hunting; by the late 1770's, if not earlier, Kotsoteka power domains were based on control of access to European goods, primarily war booty.

In 1786, the New Mexico Kotsoteka organization, consisting of eight local bands (Thomas 1932), organization was encapsulated by the Spanish in the so-called "Comanche Peace" which formalized the relationship between Comanches and Spaniards. From 1786 through the 1820's, the Kotsoteka divisional organization was maintained by the political gifts coming through Santa Fe, and by

the control of trade on the Plains. The strength of the organization is particularly evident in the ease of succession of elected Principal Chiefs, 1786 - 1818 (see Table 5).

But at the same time, Texas Spaniard relations with the Kotsotekas in Texas (the so-called "Comanche Oriental") were unsettled. The Spaniards were chronically unable to maintain a constant supply of goods to the eastern Kotsoteka, either as political gifts or through trade. There were Comanche efforts to arrange permanent trading relations as in New Mexico, as well as Spanish attempts to set up a Texas Comanche division organization with a Principal Chief, but without success.

Meanwhile, the trade between Comancheria and the east, based on horses stolen by the Comanche, undermined peaceful relations with the Spaniards. The inability of the Texas Spaniards to deter these raids made the policy of political gifts appear to be giving in to extortion, which further undermined willingness to continue them.

The encapsulated Kotsoteka organization collapsed following Mexican Independence of 1821. In both New Mexico and Texas, funds for political support were often lacking, and while some trading with New Mexico continued, it was insufficient to maintain an organization. By 1832, the ethnonym "Kotsoteka" had disappeared from both New Mexico and Texas records. While there were periodic, but indirect, references to them by Anglo-Americans, by the 1870's, only 2 local band chiefs were consistently identified as Kotsoteka. [28]

The collapse of the Kotsoteka organization was followed by the emergence of two groups of Texas Comanches, the Hois

("Timber People") in the east (by 1846 called the Penateka, "Honey Eaters"), and an unnamed western group (possibly continuing the Kotsoteka ethnonym). Through its relations with the Texas Republic in the 1840's, the eastern group established a divisional organization. But this organization was shattered by the 1849 smallpox - cholera epidemic. In the ensuing factionalism, Both factions attempted to gain Anglo-American political support, [29] but such support as was available was insufficient to support the whole population. By the late 1850's, two-thirds of the Penatekas had moved north, amalgamating with the other Comanche groups along the Arkansas and Red Rivers.

On the Texas Reservation, a modest divisional organization was maintained among the 300 or so remaining Penatekas through the control of rations and other distributions. In 1859, the Penatekas were moved to the newly established Fort Cobb agency in Indian Territory. The chief's death soon after the move precipitated a period of disorganization. In 1861, Fort Cobb was occupied by Confederates, and although a treaty was signed promising political support, little was forthcoming. The 1865 Little Arkansas and 1867 Medicine Lodge Treaties stabilized the situation somewhat. But the Penateka organization finally collapsed in 1872, when Agent Lawrie Tatum denied that there was a "head and controlling chief" (OHSKA 7-A). By thus denying the existence of the position, Tatum insured that it would not exist in the future.

The Quahada ("Antelopes") were the last Comanche political organization to coalesce from the Kotsoteka, probably not existing

much before the 1860's. Its core was the old Kotsoteka local groups of Eastern New Mexico joined by Comanches from the other divisions. As is often noted, they had no "official" relations with any government, and therefore no access to political gifts. Their principal power domain was the trade in stolen Texas cattle. When this trade was crushed in 1873, their puhacut Isatai provided the ideological basis for Quahada participation in the Red River War of 1873 - 1875.

The Yamparikas and Jupes

In the early eighteenth century, the Yamparika ("Root Eaters") were generally north of the Arkansas river, sometimes as far east as the Wichita villages below the Great Bend. By the late 1820's however, pressures from the Cheyenne and Arapahoe forced them south of the river. The Jupes ("Timber People," not to be confused with the later "Hois") were closely related to the Yamparika, but the specifics of the relationship are unclear. As with the Kotsoteka, when the Yamparika first appear in the Spanish records, their chiefs wielded considerable power. Although they did not visit Santa Fe in the same numbers as did the Kotsoteka, the Yamparika Principal Chiefs were also supported by Spanish gifts.

More important was their position between the Arkansas and the South Canadian which gave them access to Anglo-American traders moving along those rivers. This trade was apparently sufficient to maintain the divisional organization after the collapse of the Spanish and Mexican encapsulations.

With Agent Thomas Fitzpatrick's arrival on the Arkansas in

1848, and particularly after the Fort Atkinson treaty in 1853, political support gifts again became available to the Yamparika. It is important to note that the annuities from both the Little Arkansas and the Medicine Lodge treaties were given on the Arkansas. Therefore, it was mostly the Yamparikas who could benefit from them.

While their trade was first in horses, after 1850, it was also in buffalo robes. Thus when the buffalo hunters crossed the Arkansas in 1873, touching off the Red River War, they were not only attacking the Yamparika's domestic economy, but their political economy as well. It is therefore not surprising that the Yamparika insisted that the hide hunter's headquarters at the Adobe Walls be the first target.

But neither the hide trade nor political gifts alone were able to support an organization. Thus, after Paruasemena's death in 1872, no individual could gather enough support to maintain the organization. While Cheevers enjoyed the support of the reservation officials, without the income from the hide trade, he was unable to succeed to his stepfather's position. Conversely, while Tabbananaka controlled the hide trade, his hostility to Anglo-American hide hunters precluded his receipt of political support.

The Tenewa and Nokoni

The Tenewa ("Downstream") and Nokoni ("Wanderers") present a third set of political adaptations. The first mention of the ethnonym "Tenewa" was in 1818 (Wallace 1954). There is no record of any Tenewa or Nokoni visiting the major European towns, nor

evidence of European political support for the chiefs before 1820. But since their territory centered on the Red River drainage, they filled an intermediary position in the eastern horse trade, and the eastern horse traders, Anglo-American and Spanish contrabandistas, probably dealt primarily with the Tenewa.

Many of these traders were revolutionaries from the failed 1810 and 1813 Hidalgo and Magee-Gutierrez Texas revolts. After the successful 1821 Revolution, several of these exiles attained positions of power in Mexican Texas, and brought their Tenewa connection with them. But the Mexican supply problems limited political support.

Tenewa relations with the United States were mixed. David Burnet traded with them in 1818 (Wallace 1954). It is probable that the village visited by the Dragoons in 1834 was Tenewa. Similarly, the 1835 Camp Holmes treaty was probably with Tenewa. But as in the Spanish and Mexican periods, infrequent contact meant minimal political support for the Tenewa chiefs. Thus, the major source of Tenewa political capital came from the horse trade. The Comanches who frequented Chouteau's trading post at Camp Holmes were Tenewa, as were those who visited Edward's trading settlement on Little River. Warren's trading post at the mouth of Cache Creek was at the heart of Tenewa country.

In the 1840's, the principal Tenewa chief, Pahayuko, frequented the Salt Plains of the Arkansas, selling horses to the Osages. The height of this trade was in the late 1840's during the California Gold Rush, coinciding with the increased horse raids in Mexico. When the 1853 Fort Atkinson treaty undercut the Osage's middleman role, they drove the Tenewa off the Salt Plains

(Richardson 1933:187). There is little positive mention of the Tenewa during the 1850's; they are only mentioned in Texas in connection with raids, and by the late 1860's, there was only one Tenewa local band.

There is no contemporary Spanish or Mexican mention of the Nokoni, [30] nor is there much record of Nokoni contact with Anglo-Americans until the 1860's. Many horse raids were blamed on Nokonis, but their position in the distribution network it is not clear. By the 1860's, the Nokoni Principal Chief, Quinahewi, was cultivating an Anglo-American connection, in the form of a treaty with the Confederate States. However, the Confederates were unable to provide support. Quinahewi participated in the 1865 Little Arkansas treaty proceedings, but since he was not able to control all the Nokonis, his treaty goods were withheld "subject to good behavior" (NA M234 R375-399).

After 1866, the most prominent Nokoni was Tahyahquahip, "Horse Back." He was closely involved with the Americans and often acted as intermediary between the agents and the "out Comanches." But as with the Texas Penatekas, political support alone could maintain only a small divisional organization.

Summary and Implications for Further Research

I have argued that attention to the quantitative aspects of a political system, who gets what, when, and why, can provide significant insights into the nature of that system, and particularly to changes in the system., that variation in the resource base of power domains is reflected in variation in political organization. In the Comanche case, I have argued that attention

to the differential relations between the various Comanche divisions and Europeans goes a long way towards clearing the confusions surrounding the nature of Comanche political organization, the so-called "Comanche anomaly" (Kavanagh 1985), and solving the problem of "Trace the Band." [31] Specifically, I have argued that Comanche political organization seems to have been particularly sensitive to social environmental variation. Thus the organization viewed by the Santa Fe Field Party in 1933 was more an adaptation to contemporary conditions, the Great Depression and the Dust Bowl, than the persistence of Basin Shoshonean culture.

But at the same time, while the particular resources whose variations influenced Comanche political organization were European, I am not arguing that Comanche organization was the result of European Machiavellian interventionism. From the Comanche perspective, Europeans are exploitable political resources which, although they often acted perversely, could be manipulated. From as early as 1803, if not before, to as late as 1980, Comanche politicians have manipulated Europeans, the Spanish Viceroy, the BIA, or the Oklahoma Republican Party, sometimes successfully, sometimes not, according to their own assessments of the situation and its possibilities. But the point remains: inasmuch as these manipulations involve variable political resources, their variations can have a profound impact on the state of Comanche political organization.

In calling attention to the implications of resource variation on Comanche organization, I have also called attention to

the potential of a variety of documentary sources. In the Comanche case, the Spanish and Mexican records of New Mexico, and the Béxar Archives of Texas [32], contain significant quantifiable data on Comanche - European relations. But as noted several times above, quantitative data on Comanche power domains is incomplete. So far, the most complete data concern the domains based on political gifts in the Spanish and Mexican periods. But Comanches also had relations with Spaniards, French, and Anglo-Americans on the east, both by visits to settlements there as well as through traders and other agents. Archival materials from this end of Comanchería are virtually untouched, and specific research is needed on Comanche involvement in activities on the east.

The archives also contain similar data on many other peoples. For instance, the Comanches were not the only "Allied Indians" to receive gifts from Santa Fe: Navajos, Utes, various Apaches, even Pawnees came to Santa Fe and were "regaled." As early as 1807, Cheyennes and Arapahoes sent envoys to Santa Fe asking for peace "on the same terms as with the Comanche" (SANM T-2076). In many cases, names, amounts of gifts, and ethnic-political affiliations of these visitors are recorded. [33] The Anglo-American governmental records on the Comanche are much less complete and detailed; however this may not be true of other peoples.

Notes

1) Jacobs (1950), Weiss (1979), and Kelley (1980) are important exceptions. But while they note the importance of European goods in Indian politics, Jacobs particularly, giving an extended discussion of the types of goods involved and their temporal variations, they do not follow the implications of such gifts on the Indian political systems involved.

2) On the Plains, political power often involved such non-material factors such as "puha," medicine, charisma, age, or wisdom (Albers and Parker 1971).

3) The process of transformation from an abstract class to concrete social unit is "ethnogenesis."

4) The relation between a resource and a power domain and its operating unit may be prospective as well as coincident; that is people may organize in hopes of gaining a resource as well as in order to exploit an existing one. Political manipulators often exploit this prospectivity.

5) Calling the post-horse running hunt a "communal" operation is a misnomer. Its only communal aspect is a group start, after which it was individual hunting en masse. As Ewers noted of the Blackfeet, the owners of the fastest horses got first chance at the herd, and thus deprived the poor man of the right to hunt (Ewers 1955:305). The loaning of horses as with charity enhanced both the prestige and the power of the rich.

6) Comanche war records are more concerned with prestige than with booty. While Comanches did participate in joint campaigns with the Spaniards against Apaches and Pawnees, but none were particularly profitable. In 1786, 316 Comanches captured eighty-five horses. In 1792, they attacked three small Pawnee rancheries, killed five men, and captured four women and nine children. In 1810, a joint campaign netted eleven horses (Kavanagh 1986:235).

7) Although the Comancheros were supposed to have been licensed, few records of licenses being issued have been found in the Spanish Archives of New Mexico (SANM).

8) There were several more or less permanent trading sites in Texas: Mucho Que in Borden County; Las Tecovas, northwest of Amarillo; Canon del Rescate, near Lubbock; and Quitaque, in Briccoe County (Levine and Freeman 1982).

9) By the 1780's, Pierre (Pedro) Vial, a Frenchman, was a contrabandista trader and gunsmith operating at the Taovaya Villages on the Red. (Loomis and Nasatir 1967:265). As early as 1795, the American, Phillip Nolan, was in Comancheria buying horses (Loomis and Nasatir 1967:207). In 1808, Anthony Glass spent several months in the Comanche Camps along the Red River (John 1975). In

1811, William Becknell traded with them (Inman 1898:38). David Burnet spent the winter of 1818 - 1819 with Comanches along the Red (Burnet 1851:230).

10] There was a French trading site south of the Taovaya village on the Red called by the Spaniards "El Comercio de los Franceses" (Loomis and Nasatir 1969:297). It is not clear whether this was a permanent post or simply a recognized site.

11] Holland Coffee established a post near the old Taovaya village on Red River (Richardson 1933:83 n 150), but it is not clear how long this post lasted. In 1835, Auguste Chouteau founded a post at Camp Holmes on the South Canadian, (ibid., n 151) and in 1837 established another on Cache Creek near modern Fort Sill (Nye 1937:15). Another Cache Creek post was established about 1840 by Abel Warren where the creek flows into the Red (Cliff 1924). It flourished until about 1846.

The primary Texas post was operated by the Torrey Brothers, located on Trading House Creek near modern Waco. In the late 1840's, George Barnard, who had been associated with the Torrey's, operated a post near Comanche Peak.

Comanches also visited posts outside their normal range. Edward's Trading Settlement on the Little River in the Cherokee country of eastern Indian Territory had several stores which were frequently visited by Comanches; Aird's post and Jesse Chisola's store were the best known. On the southern end of Comancheria, Benjamin Leaton had a post at Presidio del Norte in the late 1840's, although most of his business was with Apaches (Winfrey and Day 1966; 5:66). Other important points of trade were Presidio Rio Grande, and further south, San Carlos, Chihuahua. These posts were intermediate stops on the raiding routes into Mexico, and often maintained "partial treaties" with the Comanches (Smith 1972).

12] In 1872, Texans returned the favor, leading the "Great New Mexico Cattle Raid" to recover their cattle from the New Mexican buyers (Kenner 1962).

13] Juan José Montoya of Cochiti told Adolph Bandelier,

The Comanche fix a certain day and place, at which the Pueblos appear, and the trading is preceded by a council and presents. The rules of trading are fixed then.
(Lange and Riley 1966:163).

Thomas James reported:

where we arrived in the evening and were met by the head chief about two miles from the town. He appeared friendly and took the goods and deposited them in his lodge
(James 1984:131-132).

Similarly, Fisher reported

when a trader arrives at a village where he is known, he proceeds at once to the lodge of the head chief, where he receives him as his guest, and commands his squaws to unpack his ponies, and convey all his goods, blankets, and cooking kit to the lodge set apart for his reception. The chief, after giving him a feast and a smoke, begs from him anything that he may fancy and then proceeds to harangue the village in words saying ... the trader had arrived in their village for the purpose of trading with them (Fisher 1869:284).

Or, as James said,

the old Chief in whose lodge I staid (sic), entered my tent with five old Indians, and all with a grave and solemn air sat themselves down in silence... as I sat on a tobacco keg, I broke off twelve plugs and took out of a box six wampuns, variously colored, and greatly prized by the Indians ... The Chief hesitated long, but at last slowly raised his hand, took my presents and smoked the pipe ... The chief then went out into the village and proclaimed in a loud voice that all should prepare to go next morning, over to the Canadian, to trade with the Tabbahoes ... The proclamation was continued by the herald on horseback till late at night "Get up your horses and make ready to go over to the white man's and trade with the Tabbahoes. They have come a great way and brought us many good things-- the Tabbahoes are good (James 1984:134-135).

Prices were fixed by the chiefs at the beginning of the trading session. Gregg noted that

In Comanche trade the main trouble consists of fixing the price of the first animal. This being settled by the chiefs, it often happens that mule after mule is led up and the price received without further cavil (Gregg 1933:239)

14] At Warren's post on Cache Creek in the mid 1840's,

the gates were closed and secured, and presently they came in crowds to the fort to trade with bundles on their backs. After much wrangling with our interpreter, they were admitted, three or four at a time, after leaving their belt knives and hatchets outside. The chief of the band was there. He said nothing but looked at the trader. The trader looked at him a moment, then took down a bridle richly ornamented with red woolen fringe and tin stars, and gave it to him with a plug of tobacco. The chief grunted, nodded lit his pipe, and the trading went on and lasted for several days (Cliff 1924:139).

When the Bents reopened the Adobe Fort in 1848, they refused to go to the villages, insisting that the Indians come a few at a time to a "ticket window" cut in the wall through which trade was conducted. The Indians were boisterous, and it was not until an

"old chief" calmed the situation that trading could proceed (Lavander 1954).

15] Spanish support of the Comanche was based on two explicitly political decisions. On the one hand, it was decided to play off the Comanche against the Apache. At the same time, it was hoped that the Comanche would act as a barrier to the French and later Anglo-Americans on the east.

In 1786, Viceroy Bernardo de Gálvez told the Governors of the Interior Provinces that

we shall benefit by satisfying their desires. It will cost us less than what is now spent in considerable and useless reinforcements of troops. The Indians cannot live without our aid (Gálvez 1951:41).

While Americans were aware of the value of political gifts with the Comanche, they were more reticent in direct acknowledgement of it. The only mention of American political gifts I have found was James Wilkinson's 1805 comment that "to extend the name and influence of the United States ... will require considerable disbursements" (Jackson 1966; 2:229).

16] In 1838, there are receipts for \$198.75 in Comanche goods, in 1843 for \$154.59. In 1844, although a total of \$14,300.13 in goods was sent to the Torrey's trading house, there are receipts for only \$35.00 in gifts to Comanches. In contrast, 1845 shows \$544.54 in Comanche gifts (Kavanagh 1986:264).

17] The negotiations leading to the 1785 Texas treaty took three weeks, during which time "the Comanches enjoyed the best entertainment the governor could provide" (John 1975:666). There were so many visitors that a Jacelon 144 feet long by 15 feet wide was built to house them all (John 1975:694).

18] In 1856, Agent Miller suggested

in lieu of fancy cover lids (sic), carbon umbrellas, rugs coats, pantaloons, calico, and mosquito netting, I would suggest an increase in all kinds of provisions with the exception of coffee and rice which should be greatly decreased, being but little used (the latter the Indians scatter on the prairie). In place of common brown sugar, I recommend clarified ... there are too many fry pans, and not enough hoop iron. For want of latter, the pans are frequently made up into arrowpoints (NA M856 R3-467).

But there were frequent complaints about the quality of these gifts:

as to the tobacco, I do not recall making use of the expression "rotten and worthless" as it is perhaps too strong. But it was really of an exceedingly inferior qual-

ity, not suitable to send even to Indiana. It was very black and strong and so wet that it need but a gentle pressure to cause the moisture to ooze out upon your hand... I gave it the soldiers, but even the old and inveterate chewers ... could scarcely make use of it (NA M856 R3-593).

19) In 1794, Governor Chacón gave Principal Chief Canaguaipé "to pass along to his brothers two fanegas of corn, one arroba of punche, and a bundle of piloncillos" (SANM T-1404). In 1802, Sofais came to Béxar asking for "five manojas of tobacco and other little things to distribute to the other capitancillos of his division" (BA 30-491). In 1808, the gifts for Sofais' rancharía were given to Cordero "in order that it be distributed through him" (Loomis and Nasatir 1967:473).

At the councils with R. B. Marcy and R. S. Neighbors before the Texas reservation, gifts were given directly to the chiefs (Parker 1856:201). Stanley described the distribution after the Medicine Lodge councils:

Yesterday was the day of distribution of presents and annuity goods. The two tribes (Kiwias and Comanches) ... seated themselves in great circles around their respective chiefs... The goods were rolled in to the center... then the chiefs divested themselves of their cumbersome robes, took out their knives and ripped open the bales. As the bright red blankets are exposed to view, cries of admiration simultaneously burst from all quarters ... Then braves stepped up to receive the blankets and distribute them around (Stanley 1967:293).

Agent Tatum described the process in 1870:

Indian rations ... were issued every two weeks to the chiefs, who divided them by having a woman of each family sit on the ground in a circle around him with her sack, and he would divide it among them, except the beef, which was issued alive, one or more head of cattle to a chief or his representative, who had a "beef paper," according to the number of families in his band (Tatum 1970:73).

It should be noted, while similar groups in other tribes were called "beef bands", and their leaders were "beef chiefs," I have found no such designation in contemporary Comanche documents.

20) In 1812, Pedro Pino noted that possession of these symbols of authority was essential to the Comanche leadership:

In order to get an idea of the esteem in which they hold the king, one need only to note that any appointments they received are ignored unless they are confirmed by our officers (Carroll and Haggard 1942:135-136).

21) An 1810 inventory of goods in the Santa Fe warehouses intended for the Comanches includes a "baston con puño de plata"

valued at 8 pesos (SANM T-2372). Canes given in Texas were less ornate: "the batons are ordinary wooden ones with the head burred ... costing five or six pesos" (BA 17-97). An 1803 invoice of gifts for the "Northern Indians and Comanches" included "1 package with 12 bastoncitos with marked wooden handles" costing 12 pesos (BA 31-423).

22] After the 1821 Revolution, they had the "Arms of the Republic of Mexico on the obverse and an emblem of liberty on the reverse" (MANM 5-428).

23] As early as 1724, Comanche chiefs were given French flags (Folmer 1942:294). Another was reported flying at the Taovaya village on the Red in 1759, when a Spanish force was defeated by a combined Taovaya - Comanche army (Allen 1939). At the same village some fifteen years later, after concluding a Comanche-Spanish peace,

Gaignard gave the chief a Spanish flag which the chief promised to place over his door so that all Comanches might see it and know that henceforth they must not harm a Spaniard (Richardson 1933:69; Bolton 1914).

When Pedro Vial went to treat with the Comanche chiefs in 1785, he took along "seven ... flags made in New Orleans" costing 7.5 pesos, and the principal signer of the 1785 Texas Treaty received a flag (BA 17-68). The flags given in Texas in 1787 were described as "of Platilla or other ordinary linen, white ... with the staff of two and a half varas, with the Burgundian Cross" (BA 17-97). As late as 1803, Burgundian Cross flags of 1 1/2 varas square were reported in Béxar invoices (BA 31-423). In 1789, Rafael Pacheco noted that Chief Zoquine had a flag "bearing the royal arms of the King and an inscription "given ... to the Chief of the Comanche, Soquine" (Castañeda 1936; V:17). The chief who visited John Sibley in Natchitoches in 1807 "wrapped" himself in a Spanish flag before receiving an American flag. In 1825, Chief Hoyoso received a Mexican Tricolor from the President of the Republic.

24] The chiefs who signed the 1785 Texas Treaty were given Captain's uniforms (BA 17-68). Canaguaipé received an embroidered scarlet cloak at his installation in 1797. As early as 1789 there was an expressed desire for "Queretaro cloth" (BA 19-827). The 1803 Béxar invoice lists "10 complete suits of clothing of Blue Queretaro" for 126.4 pesos and "15 sombreros with false gold trimming" for 26.2 pesos.

25] In 1795, the Texas gifts included 12 fusils, 75 pounds of powder, 150 pounds of lead, 100 flints, and 100 gun rods (BA 25-261) and a gunsmith was hired to repair Indian guns (BA 26-557). In 1798, 8 fusils and an escopeta (musket) were given (BA 29-487), in 1800, at least 6 fusils (BA 29-403; BA 29-481). Castañeda (1936; V:128) reports that in 1800, 100 English rifles at eight pesos each were ordered from Gilbert Leonard, trader at

New Orleans. Apparently the Comanche preferred a particular caliber: shipments of guns in both 1806 and 1808 were of the wrong size and were not accepted by the Indians (John 1984:359).

26) In 1807, John Sibley gave a medal to the Principal Chief visiting him, and had the others measured for suits of "scarlet faced with black velvet and trimmed with white buttons" (NA M271 R2:431) In 1821, James says Cordaro (sic, Cordero) was

dressed in the complete regimentals of an American Colonel, with blue coat, red sash, white pantaloons, epaulets and sword (1984:75).

In 1856, Agent Miller on the Arkansas, wanted

18 coats and pantaloons ... ornamented with the military buttons and lace which will serve as a mark of distinction for the head chief (NA M856 R3-467).

27) In 1807, John Sibley gave his Comanche visitors 6 guns, plus powder and lead (NA M217 R1-435). In 1844, J. C. Eldredge took powder, lead, and percussion caps to Pahayuko's village (Winfrey and Day 1966; 1:179). The Torrey's trading house stocked powder, lead, and some guns, and powder and lead was issued on the Texas Reservation. In 1856, Agent Miller suggested that "because of the smaller game, send flint lock rifles of 1/2 oz or 'trade ball' but not unless can supply all the soldiers (sic warriors), for the large tribes, 2-300 each" (NA M856 R3-467). Thus, in 1858, 200 flint-lock guns were bought for the Fort Atkinson treaty goods. In 1867, some 50 revolvers were given at the Medicine Lodge councils. It should be noted, there is little to substantiate the charges that Indian agents were "arming" the Comanches with better arms than possessed by the Army (see Unrau 1964). Furthermore, by the 1870's, almost any other rifle was better than the Springfields used by the army.

28) In this regard, it is interesting to note that several otherwise well informed Comanches deny the existence of a separate group called the "Kotsoteka," saying that "all Comanches ate buffalo." Conversely, at the same time that they deny the distinctiveness of the Kotsoteka, they claim that the Quahada, first named in the 1860's, were one of the primordial groups.

29) In 1852, a Texas agent wrote:

There is evidently a great want of a proper governmental organization amongst these bands of Comanche. Although originally from the same tribe (the Honey Eaters) they are now divided into small parties under different leaders. Each one considering himself entitled to equal respect and requiring a separate audience. They are convinced, however, of the necessity of having one responsible chief and each one is aspiring to the dignity. Each urges his claim with all becoming modesty and expressing a willingness to submit to an election, yet all are evidently afraid to bring the

matter to the test. (NA M234 R859-1069).

30] The earliest reference to the Nokoni in Spanish is in Garcia-Rejon's 1865 vocabulary, long after the period of intense Comanche-Mexican interaction.

31] This is not to argue that ideology, the shared cultural understandings of the nature of the world and how to get things done in it, is unimportant in political analysis. Rather the two are complementary, ideology is the "why" to the quantitative "what." In Comanche politics, questions of the control of access to resources, that is of political power, are approached pragmatically: if it works, use it. In contrast, in the Northern Arapahoe political system, age is the only legitimate source of political power (Fowler 1982). Attempts to create new power domains, pow-wow clubs, AIM chapters, etc., which do not acknowledge this power are bound to failure.

32] The Spanish and Mexican period archives of New Mexico have been catalogued and published several times. R. E. Twitchell's two volume catalogue, the Spanish Archives of New Mexico (SANM), covers the period 1621 - 1821. Volume I deals with land issues in New Mexico, Volume II contains all other documents. Each volume is arranged chronologically (mostly), and numbered consecutively within each; this number, called the "Twitchell number," is here referenced as "T-." All documents from the SANM cited here are from Volume II and so I have dispensed with the volume number in the citations.

Between 1938 and 1941, the Historical Records Survey of New Mexico made photostatic copies of the Spanish and Mexican period archives of New Mexico, as well as of documents relating to New Mexico from the major archives of Mexico. Copies of this collection, labelled New Mexico Archives (NMA) are available in several major research libraries in the United States. The Spanish period documents were arranged according to Twitchell number.

In 1968, the State Records Center of New Mexico published a microfilm edition of the of SANM. Except for reel 21, which consists of documents recovered after Twitchell made his catalogue, it was photographed and arranged by Twitchell number. For text citations here, I have used the Twitchell number, noting the microfilm roll and frame number in the References.

Documents from Mexican period New Mexico are more difficult to deal with. Since there was no precedent format for the Mexican period documents, the 1938 - 1941 Records Survey arranged them in chronological order. NMA citations give the year and page number (i.e. 1829-460). A partial calendar listing of the documents was begun, but was completed only for the years 1821 - 1830. It is at present in drawers labeled "Mexican Archives of New Mexico" in the Special Collections card catalogue (Coronado Room) of Zimmerman Library at the University of New Mexico.

In 1969, the New Mexico State Records Center published a

microfilm edition of the Mexican period documents as the Mexican Archives of New Mexico (MANM). However, in contrast with the NMA edition's straight chronology, the microfilm edition was arranged by major subject headings. Furthermore, there is no calendar listing of the documents in the MANM, nor cross reference between the location of documents in the MANM and the NMA.

The B  xar Archives at the University of Texas (BA) is here cited in the text by reel and frame number. The three volume Guide to the Microfilm Edition of the B  xar Archives provides a general listing of the contents of each roll, and there is a calendar listing of documents at the head of each roll.

33] Surviving records of the Kiowa Agency, 1869 - 1901, are available on microfilm at the Oklahoma Historical Society (OHS-KA). There is no guide to the collection, nor calendar of documents. Where possible, they are cited in the text by roll and frame number. Several rolls have no frame numbers, they are cited in the text simply by roll number and a sequential letter referring to the documentary information (author, date, etc.) listed here.

The National Archives and Record Service (NA) in Washington has microfilm publications covering many aspects of the present topic. Of particular importance are the records of the Bureau of Indian Affairs (Record Group 75), and of the U. S. Army, Continental Commands (Record Group 393). Within these major record groups, specific microfilm publications are distinguished by publication number:

- M234 Indian Office, Letters Received;
- M271 Letters Received by the Secretary of War relating to Indian Affairs;
- M856 Records of the Central Superintendency;
- T-21 Records of the New Mexico Superintendency;
- M1072 Letters sent by the military division/department of New Mexico;
- M1088 Letters received by the military division/department of New Mexico.

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Refugio
1847 - 1857
\$18,107.00
[average \$1646 per year]

Nueces
1849 - 1861
506 horses 12,800.00
253 cattle 1,265.00
[average 42 horses
21 cattle
\$1172]

Montague
1857 - 1861
135 horses 22,090.00
18 cattle 221.00
[average 33 horses
4.5 cattle
\$4462]

Table 1.1 Losses to Comanches in Refugio, Nueces, and Montague
Counties, 1847 - 1861

Cnty	1865-1867		Prior to 1873		1873 to 1875	
	Horses	Cattle	Horses	Cattle	Horses	Cattle
Archer					7	
Bandera	135				62	
Bexar			25			
Blanco	185					
Bosque	94					
Brown					33	
Burnet	3		254		10	
Callahan					137	250
Clay			57	1,010	670	
Coleman						
Comal	16				24	
Comanche	75					
Concha	28					
Cooke	154	1,308				
Denton			1,195			
El Paso	110	1,168			10	
Erath	88				50	
Gillespie	40	1,000	1,177		66	
Hamilton	215		500			
Hood	46					
Jack	100	5,000	103		300	
Kendall	137		705	25		
Kenney					40	75
Kerr	70				182	710
Kinble				2,650	24	
Lampasas	409	400				
Live Oak			480			
Llano	37	151				
Llano	44				35	
Mason	38	500			10	
Maverick					4	
McCulloch	44	827			4	
McCulloch		4,000				
McMullen			474			
Medina	182					
Menard	57	2,720	562	5,000	271	
Montague	191	1,100	32			
Montague		11,625				
Palo Pinto	150		64		130	
Parker	454					
San Saba	75				476	125
Shackelford			37		25	
Stephens					98	
Tarrant	16					
Tom Green					445	
Uvalde	1,198	3,085			186	
Webb	88					
Wichita					12	
Wise			441		340	
Young			92		88	
Zavala					70	
Totals:	4,479	32,884	6,198	8,685	3,809	1,160

Table 1.2 Reported Losses in Texas, 1865 - 1875. (Kavanagh 1986:354)

Year	On Hand	Received	Total	Expenses	Balance
1789				5906.0.00	
1790	289.4.01 1/2	4000.0.00	4289.4.01 1/2	5712.2.02 1/2	-1423.2.01
1792	2794.3.08	4900.0.00	7694.3.08	3882.7.11	3371.3.09
1794	3061.4.01	4500.0.00	7561.4.01	3299.5.09	4261.6.04
1796				3323.1.01 1/4	-1615.8.00
Jan - Oct 1797				4216.4.03 1/8	
1797-1798	1767.2.11 5/8	3000.0.00	4767.1.11 5/8	3327.9.09 5/8	1439.9.05
1799-1800	2067.0.11 7/8	2900.0.00	4967.0.11 7/8	1072.4.04 1/2	3494.4.07 1/8
1800-1801	3494.4.07 3/8	1000.0.00	4494.4.07 3/8	1924.1.00	2570.3.04 3/8
1801-1802	2570.3.04 3/8	800.0.00	3370.3.04 3/4	2826.0.07	544.2.09 5/8
1803-1804	880.7.00 3/8	2000.0.00	2850.7.00 3/8	3599.5.07	-748.6.06 5/8
1804-1805	-748.6.06	6737.5.00	5988.9.00	5988.9.00	-0-
1810-1811	1131.1.03	5696.4.01	6827.6.01	5632.3.09 1/2	1195.3.03 3/4
1811-1812	1195.3.03 3/4	4864.3.10	6099.7.01 3/4	5847.2.06 1/2	212.4.07 1/4
1812-1813?	3701.3.08 3/4	4648.0.00	8349.0.00	1199.4.04	3093.5.09
1814-1815	720.0.05 1/4	2620.2.11	3340.5.04 1/2	4949.6.02 3/4	-1809.0.30 1/2
1816-1817	2140.1.08 3/4	1500.0.00	3640.1.08 3/4	3889.2.08 3/4	-249.0.11 3/8
1818-1819		6340.0.00	6340.0.00	4761.2.03 1/8	1579.5.02 7/8
1819-1820	1061.4.11 7/8	1429.4.10	2491.1.09 7/8	2589.9.09 3/4	-94.3.11 7/8
Total				73948.2.08 3/8	

Table 2.1 Gastos Extraordinario de Paz y Guerra, Santa Fe,
1789 - 1820. (Kavanagh 1986:260)

=====

Year	Total in Pesos	Comanche Share
1789	5906.0.00	4248.0.00
1790	5712.2.02 1/2	1769.7.10
1791		
1792	3882.7.11	1248.8.11
1793		
1794	3286.1.09	1110.4.07 3/4
1795		
1796		
1797	4216.4.03 1/8	2602.8.07 3/4
1798	3327.9.09 7/8	1366.6.03 3/4
1799		
1800	1072.4.04 1/2	537.9.07 3/4
1801	1924.1.00	992.3.03
1802	2826.0.07	1833.5.10
1803		
1804	3599.5.07	1436.0.00
Total	35,753.3.06	17,146.5.05 (48%)

Table 2.2 Comanche Share of Gifts, Santa Fe, 1786- 1804 (Kavanagh
1986:262)

Date	Visitors	Gifts in Pesos	
		Total	
1787 January	107 Comanches and Taovaya	340.5.00	
April	Comanches and Taovaya	83.6.00	
May	Comanches and Taovaya	418.0.00	
			842.1.00
1788 January		438.0.00	
			438.0.00
1796 January - April		601.0.00	
May - August		620.0.00	
September - December		1188.0.00	
			2609.0.00
1798 May	144 Comanches and Taovaya	689.3.03	689.3.03
1799 [month?] 281 Indians		230.6.11	230.6.11
1794-1799 [Total for 5 years]		8849.0.00	8849.0.00
1800		3194.5.00	3194.5.00
1806	2056 Comanches, 4497 total	3228.0.00	3228.0.00
1810	1374 Comanches, 2425 total	3905.6.04	3905.6.04
Total			23986.5.06

Table 3.1 Indian Expenses, Bexar, 1787 - 1810. (Kavanagh 1986:261)

	Comanches/all others	Gifts in Pesos
Jan	76/389	296.0
Feb	248/299	216.4
Mar	286/294	272.1
Ap	169/494	297.1
May	7/290	299.0
June	74/170	319.2
July	376/529	190.0
Aug	306/429	191.0
Sept	224/562	296.6
Oct	239/799	198.7
Nov	30/206	49.7
Dec	17/36	603.0
	2056/4497 (45x)	3228.4

Table 3.2 Comanche visitors to Bexar, 1806 (BA 34-219).

	Comanches			Total	Gifts in Pesos
Month	Male	Female	Total	Visitors	
January	56	4	60	122	394.2.11
February	131	68	199	230	287.3.10
March	229	179	408	422	708.3.06
April	7	1	8	133	139.4.07
May	1	1	2	22	37.4.03
June	2	3	5	129	97.0.00
July	64	53	117	495	430.5.06
August	0	0	0	7	294.2.05
September	51	38	89	329	137.5.01
October	302	18	486	486	394.5.01
November	0	0	0	50	147.7.04
Total	843	531	1374	2425	3905.6.04

Table 3.3 Comanche Visitors to Bexar, 1810. (Kavanagh 1986:261)

1835 Camp Holmes		\$10,000
1846 Butler - Lewis		10,000
1853 Fort Atkinson	\$18,000 @ 10 yrs	180,000
1865 Little Arkansas		
until all settled on Reservation,	\$10 per capita	
after that	\$15 per capita	
for forty years		
1867 Medicine Lodge for 30 yrs,	\$25,000	75,000
one time gifts		
(left over from Little Arkansas)		120,000

Table 4. American Funds intended for the Comanche and Allies,
1835 - 1870

1) 1786-1830

New Mexico Kotsotekas

Ecueracapa 1786-1793	Encanguane 1793-1797	Canaguaipé 1797-1801	Quegue 1805-1818	Cordero -1826	Toro Echicaro 1829-?
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Texas Kotsotekas or Comanche Oriental

Canisa de Fierro and Cabeza Repada -1786	Zoquine and Sofais 1786-	Chihuahua 1801-1806	Cordero 1806-1812
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Yamparika and Jupe

Toro Blanco -1785	Paruanarinuco ?-1786-1797-? [Jupe]	Soniquaso 1805-?	Paruequita (Oso Ballo) ?-1810-1826-?
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2) 1830 - 1875

Kotsoteka

Noway
-1875

Yamparika

Shaved Head - 1857	Bistevana -1861-	Paruasenena -1872
-----------------------	---------------------	----------------------

Penateka

Muguera -1840	Mozechucope -1849	Sanaco 1849-1855	Ketunsee 1855-1861	Kahabbywite (Kaharawa) 1861-?	Tosawa 1865-1875
Potsanequahip 1849-1862-?					

Tenewa

Paruequiviste 1821-1832	Tabbaquena ?-1834-1839-?	Pahayuko ?-1844-1855-?	Boyaquasso ?-1865-1875
----------------------------	-----------------------------	---------------------------	---------------------------

Isaony
?-1835-1837-?

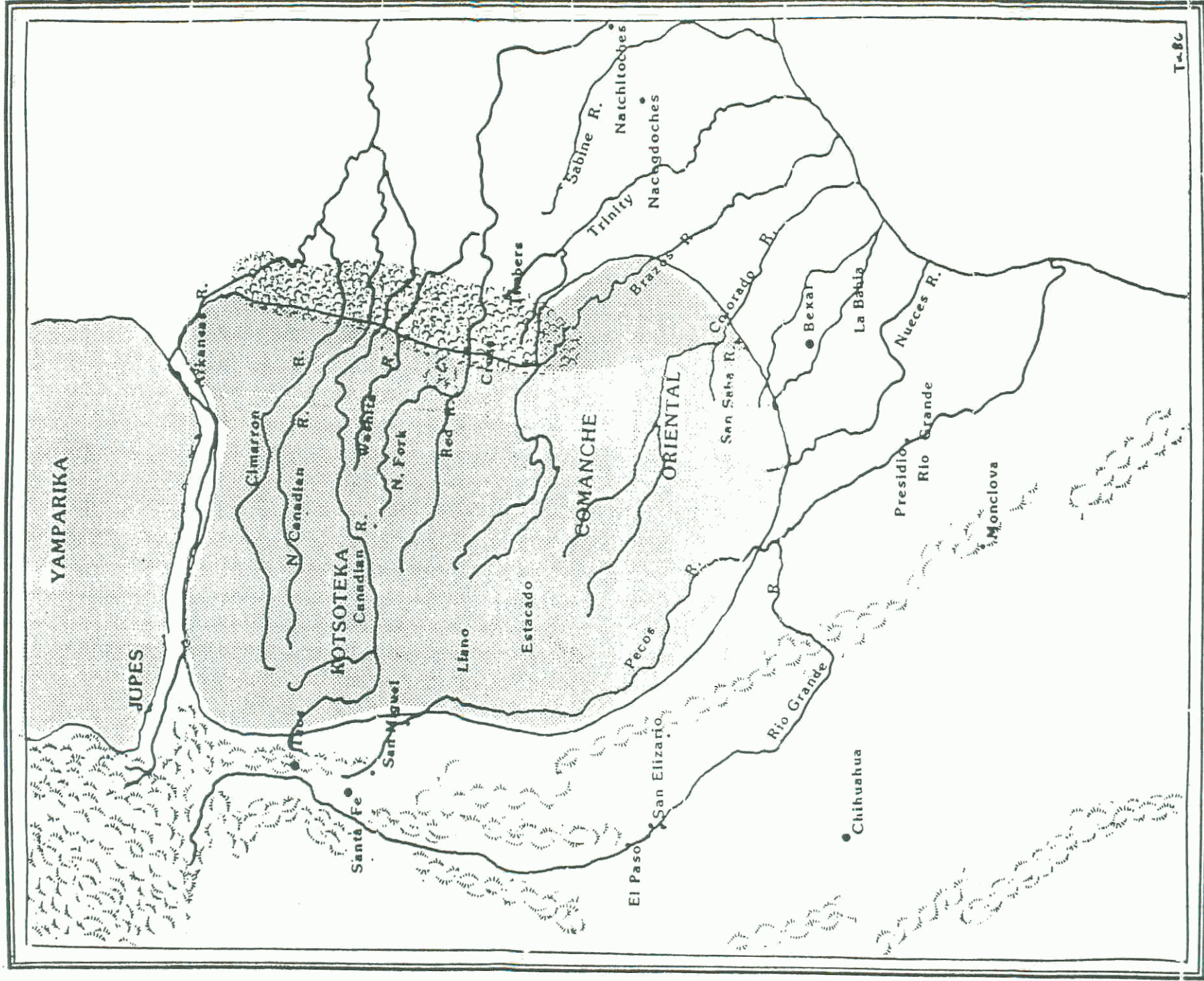
Nokoni

Guinahewi ?--1861-1866--?	Tahyahquahip --1875
------------------------------	------------------------

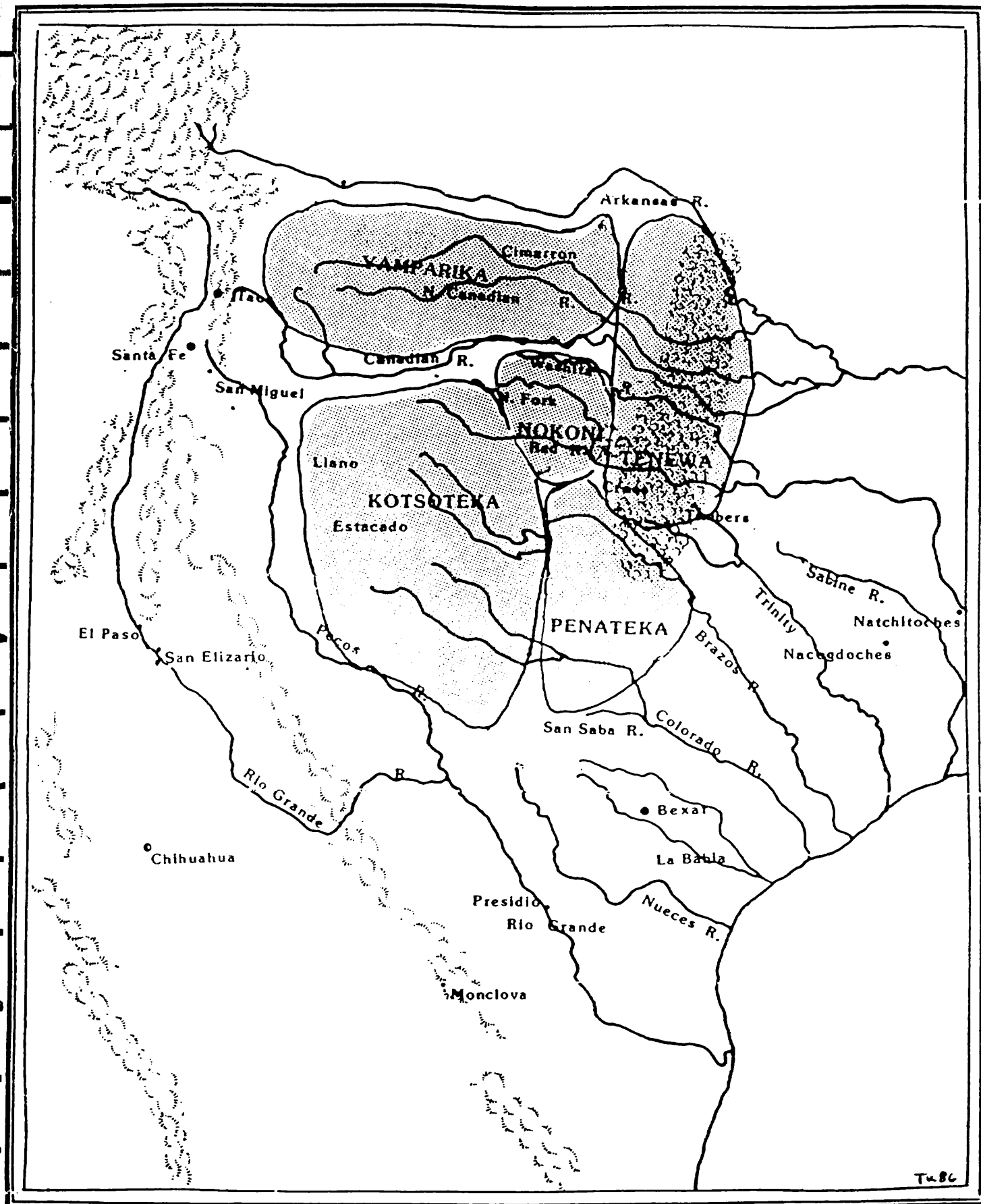
Table 5. Comanche Principal Chiefs, 1786 - 1875

	General con Casacas Escarlata	Capitan Con Capa	Capitan Sin Capa	Gandul	Muger
Varas Paño (woolen cloth)	7 1/2	8 3/8	2 3/8		
Varas Bayeta Azul (blue bayeta)				1 1/2	
Varas Bayeta Tlascala (Tlascalan bayeta)	1	1 1/2	1/2	1/8	
Botones (Buttons)	3 doz	11	11		
Varas Manta (blankets)	13 1/2	9 5/8	9 3/8	4 1/2	
Sombreros	1	1	1		
Belduques (knives)	1	1	1	1	1
Piloncillos (hard sugar cones)	1	1	1	1	1
Liston No. 40 (ribbon)	1	2	2		
Cigarros	1	2	1	1	1
Abalorios (beads)					10

Table 6. "Extract of the Quantity of Gifts for Each Class," 1807.
(SANM T-2084)



COMANCHERIA ca. 1785



COMANCHERIA ca. 1846

change over time is obscured. As Kavanaugh has shown us the stability of various levels of political organization among the Comanche was intimately linked to changing resource bases. This approach to political analysis should be fruitful for tribes other than the Comanche, because it allows the specification of whatever the pertinent resource bases might be.

The necessity to systematically specify resources is both the strength and weakness of this approach. Without the data, little can be accomplished. But Kavanaugh has brought to our attention important sources of data for the Comanche and other tribes in the records of Spanish, Mexican, and Anglo administrators. These economic data--on gifts, thefts, trade, and allotments--provide a much needed quantitative base for the analysis of political exchange. They are, as Kavanaugh notes, particularly useful for the Comanche because of the instrumental nature of political leadership among the Comanche. As with all data sources, their limits are our limits. Because the Spanish and Mexican records seem to be much richer than the Anglo ones, these sources will always tell us more about those tribes and chiefs within areas of Spanish and Mexican influence than those of later time periods, those closer to the Anglo settlers, or those who might have avoided contact with Europeans.

Using this approach and data, Kavanaugh has presented a unified explanation for the rise and decline of bands and principal chiefs. The analysis of the scope of political gifts from prestige through subsistence items and their distribution to the different levels of leaders is especially strong. So too

Kavanaugh gives us detail of the widespread trade between Comanches and Europeans and the role of political leaders in that trade. He examines the variations over time in these and other sources of political power and describes their correlation to changes in principal chiefs and band names.

This analysis would be strengthened, however, by further attention to certain aspects of the political exchange model and more systematic analysis of the data. Kavanaugh describes the role of the chiefs in the distribution of economic resources, but despite some evidence does not emphasize their role in producing resources. This is an important source of power for leaders, and one which reflects both their own abilities and changing possibilities in the material world. For instance, he mentions that the ability of local chiefs to ensure a degree of order and security increased the willingness of white traders to venture across the Plains. Thus local chiefs who could produce this security could get more trade goods for their people. Kavanaugh also mentions that whites tried to undercut the role of the chief in their fortified trading posts. A potentially useful line of analysis would be to see how this assault on the ability of the chiefs to produce a political resource, trade, affected their power. Similarly, Kavanaugh insists that the principal chiefs, whose primary resource was political gifts, were not mere creatures of the Europeans. By analyzing how the chiefs "produced" European gifts as well as how and when the Europeans decided to give them, this point would be strengthened.

The specification of the pertinent resources for political power among the Comanche would also benefit from further

COMMENT

"POLITICAL POWER AND POLITICAL ORGANIZATION:
THE QUANTITATIVE ASPECT OF COMANCHE POLITICS,
1786-1875"

Susan E. Hirsch
Northwestern Univ.
February 20, 1987

In this analysis of Comanche politics, Kavanaugh presents an approach to political analysis and a body of data which shed new light on a central problem in Comanche historiography, "trace the band," and which should prove very fruitful for historians of other Indian tribes. In understanding the rise and decline of political leaders and organizations, the idea that politics is a process of political exchange has great power. Through delineating the resources available to leaders and those dispersed to population groups and tracing the use made of these resources, political scientists, and to a lesser degree historians, have begun to deal with causation and with change over time in a systematic fashion. Most of the work of political scientists in this vein deals with third world countries, but this approach should be one of interest to historians of white America as well those studying American Indians. Robert Dahl's superb study of the shifting centers of political power in New Haven, Who Governs?: Democracy and Power in an American City (New Haven, 1961), is virtually the only study of American politics based on this theory of politics.

This is a behavioral approach to politics, but one which is potentially more powerful than that which dominates the historiography of American political analysis--studies of voting behavior. In the latter studies, resources controlled by leaders (like economic goods and services, authority, status, information, and coercion) and those dispersed to sectors of the population (like economic goods and services, status, legitimacy, information, and violence) are conflated into attributes of voters, and the dynamic of exchange and hence the source of

explanation. First, we must ask whether there is evidence that Comanche political concepts remained constant across this time period. Was political organization always based on economic resources? We also need to know whether the role of economic resources was equally important at all levels of leadership. Kavanaugh notes that the gains made from native wars were probably not very significant economically, but that the primary goal of such wars was prestige. How important was such prestige as a political asset and at what level? A useful graphic aid for readers would be a cross tabulation of sources of power and levels of leadership among the Comanche.

The very rich information that Kavanaugh provides would prove even more impressive to the reader if it were presented with greater specification of time periods and in more varied formats. In the discussion of the role of the buffalo and the horse, for instance, the timing of the transformation of this resource from one that belonged to leaders to one that belonged to wide segments of the population is unclear. If this can be pinpointed, can it be explicitly related to band naming or changes in leadership and at what level? The analysis would benefit also from estimation of the trends of expenses for political gifts and the Comanche share of those expenses (Tables 2.1, 2.2, 3.1). A graph of such trends and of the tenure of principal chiefs would be most useful.

Furthermore, an ongoing difficulty with assessing the importance of gifts, trading, and the like is the lack of discussion of the economic condition of the Comanche. In footnote

12, Kavanaugh quotes a Spanish official as saying in 1786 that the Indians could not live without Spanish aid. In what sense might this be true? What role did the political gifts play in the Comanche economy? Similarly, Kavanaugh makes good use of the data on losses of horses and cattle to show that the usual estimates are greatly exaggerated, but he does not estimate what even this lower volume of theft might mean for the economy of the Comanche. It is also important to know whether the economic condition of the Comanche changed significantly across time, as the potential power of political domains based on gifts, or trade or theft probably also would change. A time line which displayed the state of the Comanche subsistence economy and the extent of gifts, trade, and theft would aid the reader in getting a sense of the role of these factors in the total Comanche economy.

The discussions of gifts, trade, and theft could all benefit from maps which related such activities to the existence of bands and principal leaders. I am making a presumption, which may be erroneous, that bands were likely to engage in those activities in the areas they inhabited. I think the data on theft would be particularly amenable to this treatment. A map could show whether the highest incidences of theft by county were correlated with the existence of particular bands and chiefs in those areas.

The final section of this paper relates the history of the principal chiefs and divisional organizations to the information on various types of political resources. The proliferation of named groups in response to the increase in types of resources and in the sources of their distribution is clear. As with more narrow points of the analysis, however, the the argument would be

more firmly established by better presentation of the data. Table 6 needs to be reorganized so that the succession of groups is clear. The table also is incomplete, as several chiefs and one group which are mentioned in the text do not appear on it. The suggestions made in the above paragraphs relating types of resources to specific chiefs and named groups would further bolster this overall argument. Although the data do not seem amenable to more sophisticated mathematical modeling, they can be analyzed more usefully with simple statistical methods. The data Kavanaugh has uncovered and the method he has proposed for studying Comanche politics will bear more fruit when used more systematically. But Kavanaugh has given us a new and useful way to look at a long-standing problem as well as a model for political history which has been too long ignored by historians.

Locality as a Factor in the Band Organization
of the Osage Indians: A Quantitative Analysis

by
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Introduction.

The use of Indian Allotment Schedules to spatially isolate sociologically significant groups was an early objective of the Cheyenne Ethnohistory Project and led to the illustration of the role of a uterine/agnatic dialectic in Southern Cheyenne local organization (Moore 1980, 1981). Additional analysis of Southern Cheyenne allotment selections has demonstrated the further utility of this methodology in the analysis of residence patterns at the familial level (Nespor 1981).

The initial attempt to apply a similar research design and methodology to the Osage case was intended to produce comparative material for the Cheyenne situation through the examination of an emphatically patrilineal society. Analysis of the 1906 Osage Allotment Schedules resulted in preliminary conclusions regarding the local organization of the Osages in the early twentieth century (Swan 1981).

Impetus to renew this line of research came from the fortuitous "discovery" of a schedule of land claim selections filed by Osage family heads in 1875 (Office of Indian Affairs- Letters Received-Osage Agency 1875. Hereafter cited as OIA-LR-OA). These claim selections will be used in this paper as a potential source of information on the local organization of the Osages in the late nineteenth century. Specifically the relationship between Osage "Bands" and the named physical divisions of the tribe will be examined. The reorganization of Bands for subsistence strategy

differentiation will be suggested as an important factor in the spatial organization of the tribe during this period.

Research Setting.

In 1873 Osage Agent Isaac T. Gibson divided the reservation under his charge into five administrative districts; Agency, Bird Creek, Hominy, Salt Creek and Little Osage. The agent provides the rationale for this action in his statement that (Annual Report-Commissioner of Indian Affairs-Osage Agency 1873: 216. Hereafter cited as AR-CIA-OA):

"Each division is in the charge of an efficient farmer and assistants, who reside at a station most central and convenient for the Indians in his care, ... This method has worked admirably, bringing all of the Indians to some extent under the influence of the agent. These Divisions are being provided with necessary dwellings for the employees at the stations, none of which are nearer the agency than fifteen miles, and some of them are fifty miles from each other.

The establishment of administrative districts and agency substations was a direct response to the dispersed nature of Osage settlement and the five districts correspond to named physical divisions within the Osage Nation. The Osage attribute these named groups to the accidental division of the tribe when they were forced to flee their village during a flood (Mathews 1968:143-148) . The divisions are named for

the topographic features of the locale in which the new-formed groups found themselves in after the flood. Several translations and orthographies exist for these names and those of La Flesche will be used herein: Pacingthin, "The Dwellers Upon the Hill Top" (Salt Creek); Con-dse-gthin, "The Dwellers in the Upland Forrest (Hominy); Wa-xa-ga-u-gthin or "The Dwellers in the Thorny Thicket" (Agency); I-udse-ta, "The Dwellers Below" (Little Osage); and Non-dse-was-pe, "The Heart-Stays" (Bird Creek) (La Flesche 1921: 45; 1932: 124, 32, 207, 81, 141).

This division of the tribe did not alter the socio-ceremonial organization of the Osage as each division contained a full representation of the clans of the tribe (LaFlesche 1921: 45) and economic and political autonomy is also supported for each division (Bailey 1974: 43-44). The number of physical divisions was reduced to three when the Non-dse-waspe and the I-udse-ta consolidated with the Wa-xa-ko-lin prior to 1884 (Bailey 1974: 151; La Flesche 1921: 45).

The Osage Agency, Hominy Substation and Salt Creek Substation provided the foundation for the modern day communities of Pawhuska, Hominy and Gray Horse respectively. The 1906 Osage Allotment Act reserved 160 acres from selection near each of these communities for those members of the tribe who wished to continual a communal existence. Today these "Indian Villages" continue as "mini-reservations" within Osage County, Oklahoma and "District" affiliation remains an important organizational principle in

contemporary Osage society.¹

These five divisions have been stated to have existed "aboriginally", with each division corresponding to a village unit of coresidence (Bailey 1974:54-55, Chapman 1974: 51, Mathews 1965:698). This position is contradicted by various interpretations of the historic record regarding Osage villages and local divisions in which the number of villages increases over time as a consequence of the deterioration of the traditional political organization of the tribe (Bailey 1974: 106-109; Chapman 1974: 89-92; Mathews 1964: 694).

The term "band" has been used to distinguish these "splinter groups" from their respective physical divisions, with the appearance of a new band taken to represent the establishment of a separate village. The number of Osage "bands" and their respective relationship to the named divisions of the tribe is a source of considerable confusion. When the Osage settled on their reservation in Indian Territory in 1872 they are reported to have been dividied into between seven and seventeen bands (Bailey 1974:135). La Flesche makes no mention of the band composition of physical divisions and locates the tribe in three villages on their reservation (La Flesche 1921:45). Mathews states that there were five villages established on the reserrvation but his description would indicate seven villages, some containing more than one band (Mathews 1964: 698-699). The most comprehensive effort to correlate individual "bands" with a physical division is found in the work of Louis Burns (1984:

15-17). Working from the bands contained in the 1877 Osage Annuity Roll Burns produced a map which locates each band within the reservation (ibid: 9). Burns makes no mention of the actual number of residence groups or villages and had trouble placing some of the bands in a physical division.

This paper will address these issues through the examination of Osage local organization in the late nineteenth century. The 1875 claim selections will be subjected to a series of computer mapping techniques to isolate the relationship between physical divisions and bands and to determine the spatial dimensions and composition of these social groups. Differing subsistence strategies will be shown to provide economic and ecological criteria for the distinction between two forms of geographic patterns displayed by Osage Annuity Bands.

Data and Methodology.

Following their relocation in Indian Territory the Osage were subjected to various policies and programs designed to integrate them into the economy of the dominant society. The transition to private land ownership and the adoption of cash agriculture as a major subsistence activity were central features of this goal (LR-OIA-OA:1874,1875, 1876). To document the progress of these programs the Osage Agent began to accumulate statistical information concerning the demographic, economic and locational condition of the tribe. The period of 1874 to 1878 was thus one in which detailed quantitative data becomes available for the Osage.

The 1875 Claim Data includes the name of the family head, the legal description of the selected parcel of land, the number of acres fenced and cultivated and additional information regarding improvements to the claim. The Claim Data was organized by the "sub-agency" affiliation of individual family heads. The relationship between these agency stations and the named physical divisions of the tribe was discussed in the preceeding section of this paper. The major assumption made here is that claim selections were made by the Osages themselves and that they provide a meaningful representation of the spatial dimensions of Osage residence groups. The schedule of claim selections contains an entry for 221 family heads and is treated in this work as a representative sample for the tribe.

The second major data source used in this analysis is the 1876 Osage Annuity Roll (LR-OIA-OA: 1876). Proceeds from the sale of their Kansas Reservation were placed in a Government Trust with interest on the account disbursed in quarterly payments. The roll was updated each quarter by adding births and deleting individuals whom had died during the preceeding three months. The Roll was organized by band affiliation, and contained the roll number, name of head of household, the number of men, women and children in the household and the amount payed to each individual. The term "band" will be defined in this paper as a social group which is politically organized for economic production and consumption. Band membership provided access to annuity

goods and trade items during this period. The Band Chiefs would receive the annuity goods for their respective members and then distribute these materials according to "Indian custom" (LK-OIA-OA 1875).

The first stage of data analysis will code the claim data for entry into the computer. Data sets will be structured according to the format configurations of the Statistical Analysis System (SAS). The legal descriptions of individual claim selections will be converted to a single numeric value through the application of a matrix coding scheme and conversion formula developed by Robert Nespor and Stan Johnson of the Cheyenne project (Nespor, op.cit.)² Subdivisions were most often in the form of quarter sections⁴ but the coding scheme is capable of treating any form of subdivision encountered (Figure 1a.) The resulting subdivision codes will then be combined with the section, township and range of the legal description to produce "x" and "y" coordinate values for the geographic center of each claim selection (Figure 1b).

The next stage of data analysis will assign different categorical values to each of the data points produced by the "x-y" coordinate formula. This will provide a test of the ability of Osage Physical Division and Band affiliation to explain the patterns observed in the 1875 Claim selections.

The subagency affiliations were obtained from the 1875 Claim Data with Band affiliation derived by linking the 1875 data to the 1876 Osage Annuity Roll. A success rate of 75%

Figure 1a. MATRIX CODING SCHEME

Subdivision Codes	
NE 1/4 = 1	N 1/2 = 5
NW 1/4 = 2	S 1/2 = 6
SE 1/4 = 3	E 1/2 = 7
SW 1/4 = 4	W 1/2 = 8

Examples:

Legal Descriptions:

NW 1/4 of Section 16, Township 22, Range 7

Selection Code: 2162207
 QS S T R

Figure 1b. Conversion Formula

$$X = (768 * \text{Township} + 128 * \text{NSS} + \text{NS}) / 32$$

$$Y = (768 * \text{Range} + 128 * \text{EWS} + \text{EW}) / 32$$

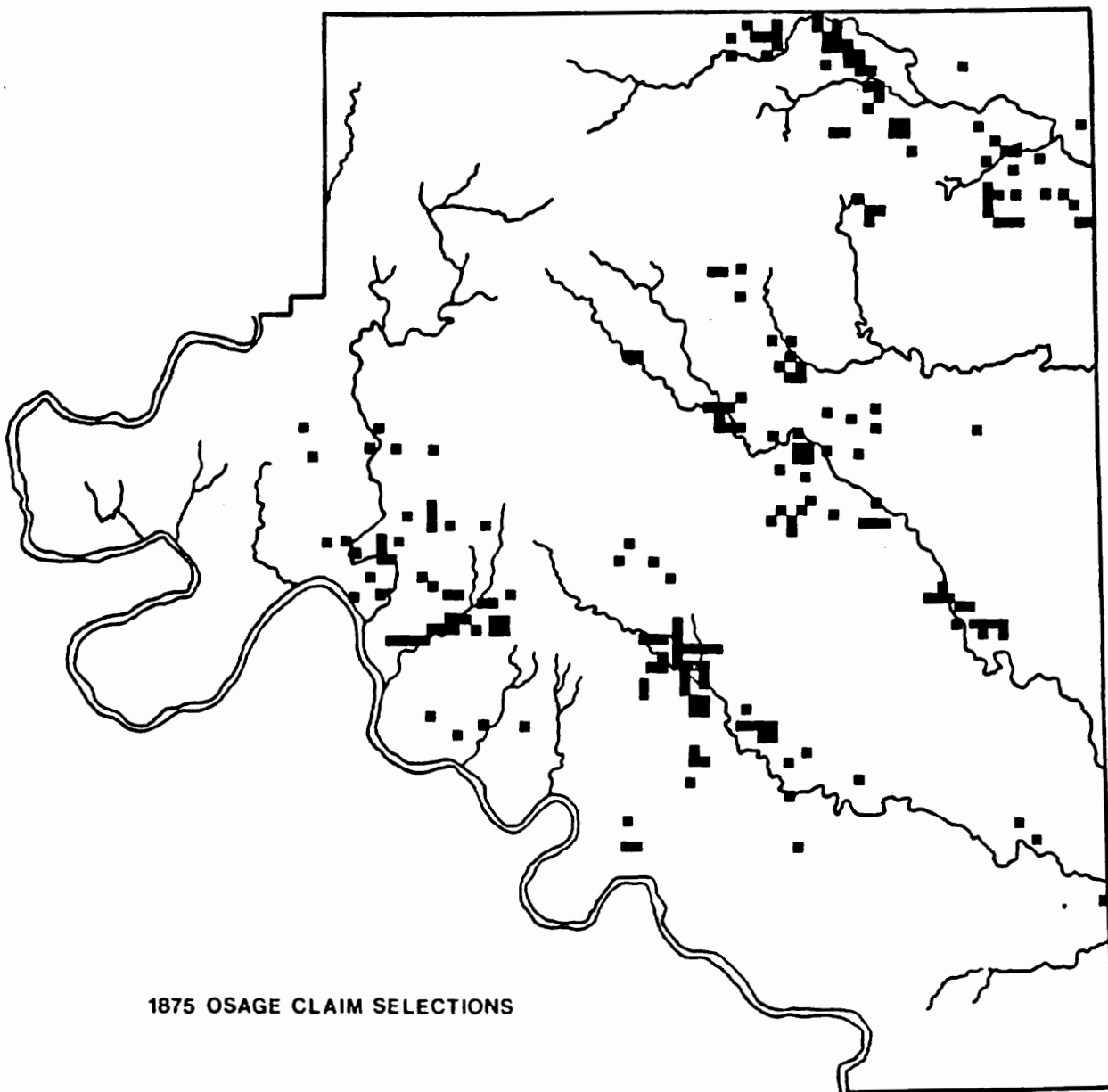
was achieved in the linkage of the 1875 Claim Data with the 1876 Annuity Roll. Rigid acceptance criteria were imposed to increase the reliability of linkages at the expense of number of links achieved. Personal name was the major linkage variable and the problems associated with Indian names are well known to those who have attempted nominal record linkage in American Indian populations. Alternate names, mistranslation and the multiple occurrence of names are the most commonly encountered problems.

Two types of computer maps will be produced during the course of data analysis. The first map type involves the application of the plotting procedure of the SAS program package (SAS Institute Inc. 1985) to produce approximations of standard plat maps. The second map-type was produced through an application of the Synagraphic Mapping Program (SYMAP) which interpolates areal or surface maps from point data (Sheehan and Dougenik 1975).

Results of Data Analysis.

Claim selections were subjected to the coding scheme and conversion formula outlined above. A manual plot of the resulting "x" and "y" coordinate values is presented in figure 2. The major river and stream channels within the reservation have been overlaid on the plot to provide an initial indication of ecological considerations in land selection patterns.³ A number of clusters of claim selections are visually discernable with varying degrees of spatial compactness exhibited.

Figure 2.



The first series of computer generated maps were point maps with the value of the point assigned according to the subagency affiliation of the individual. It can be seen in Figures 3a-3d that subagency designations are quite successful in maintaining spatial integrity. Government rations and per-capita payments on the tribe's Government Trust were distributed from these agency substations. The closing of the reserve and the loss of the buffalo as both a source of food, and more importantly, as a source of robes for trade, forced the Osage to depend more heavily on their quarterly annuities for subsistence.

If the 1875 Claim Data is viewed as a "single-frame" in the historical development of Osage local organization, then the inconsistencies in data performance might be a potential source for the isolation of the principles involved in local group formation during the period. The map for the Salt Creek subagency (Figure 3d) is noteworthy in that it contains three selections by individuals from the Hominy subagency. This is viewed as an instance in which a "single-frame" has preserved evidence of a dynamic event; in this case the relocation of individuals from one Osage physical division to another.

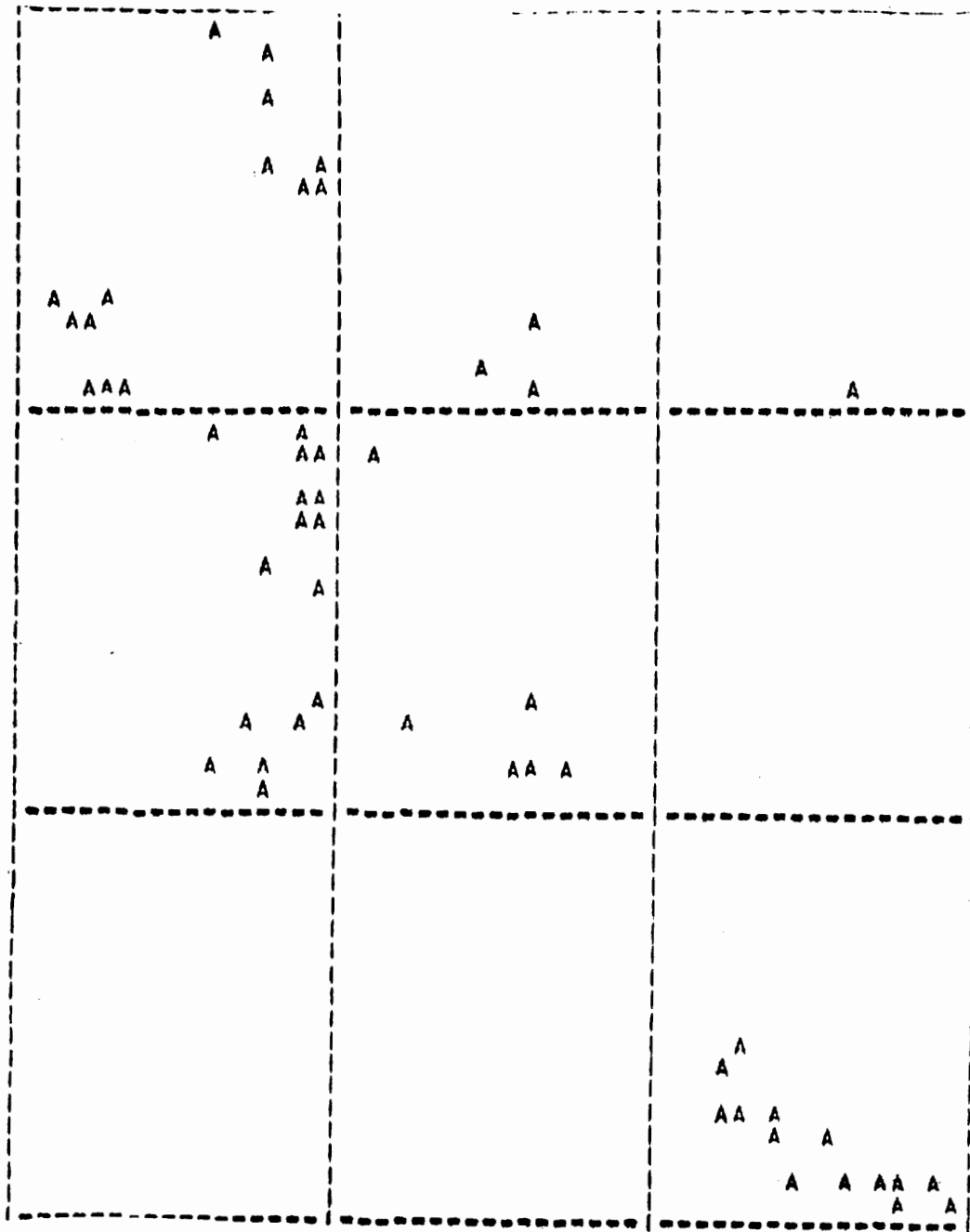
The point data utilized in the previous section can be used to produce surface or areal maps through the use of the **SYMAP** proximal map routine. This type of map displays data by assigning a value to every point in a user defined study area through a variation of nearest neighbor discriminant

[illegible]

Figure 3a.

Figure 3a.

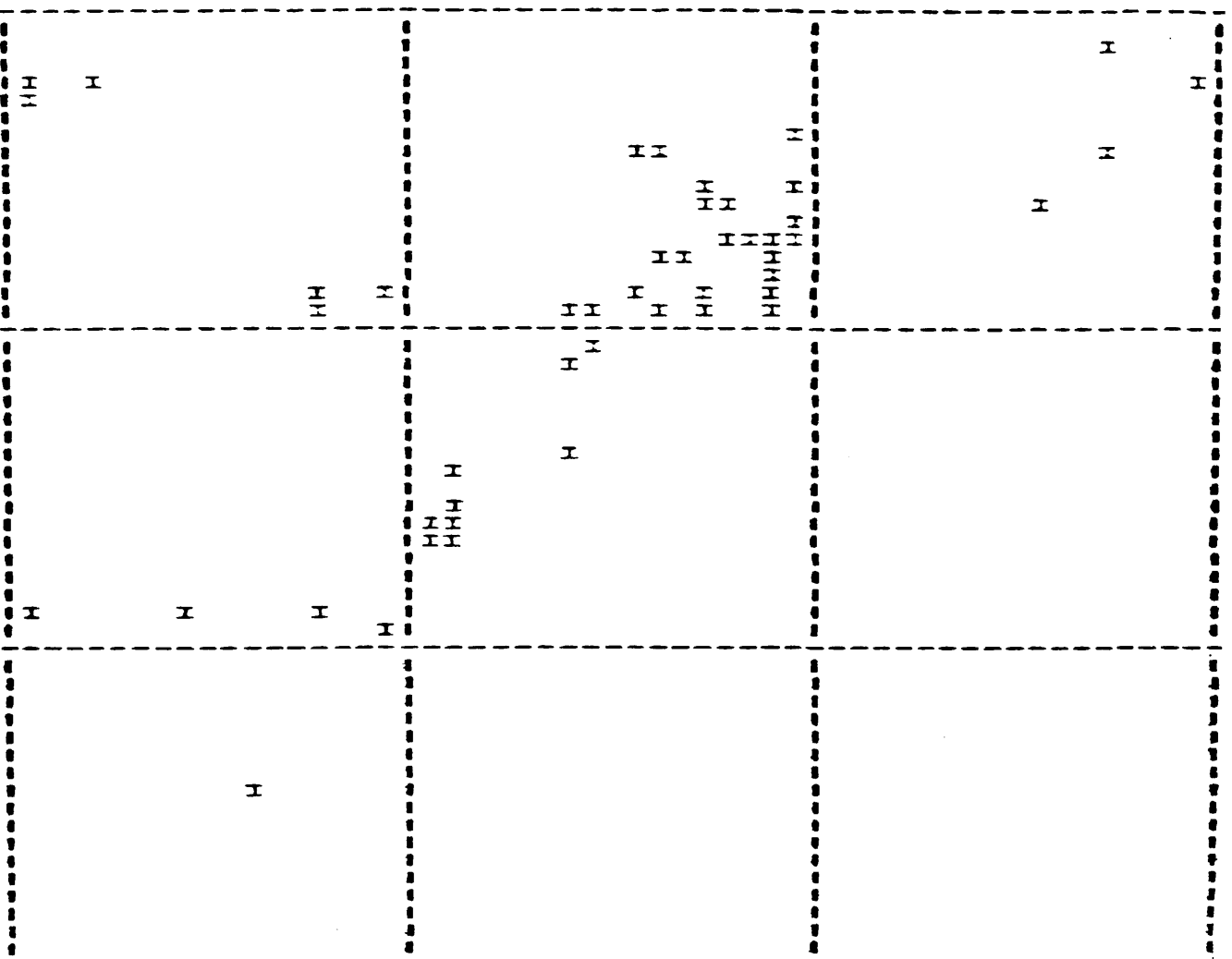
1875 Osage Land Claims- Male Famhds



Symbol is value of Station

Figure 3b.

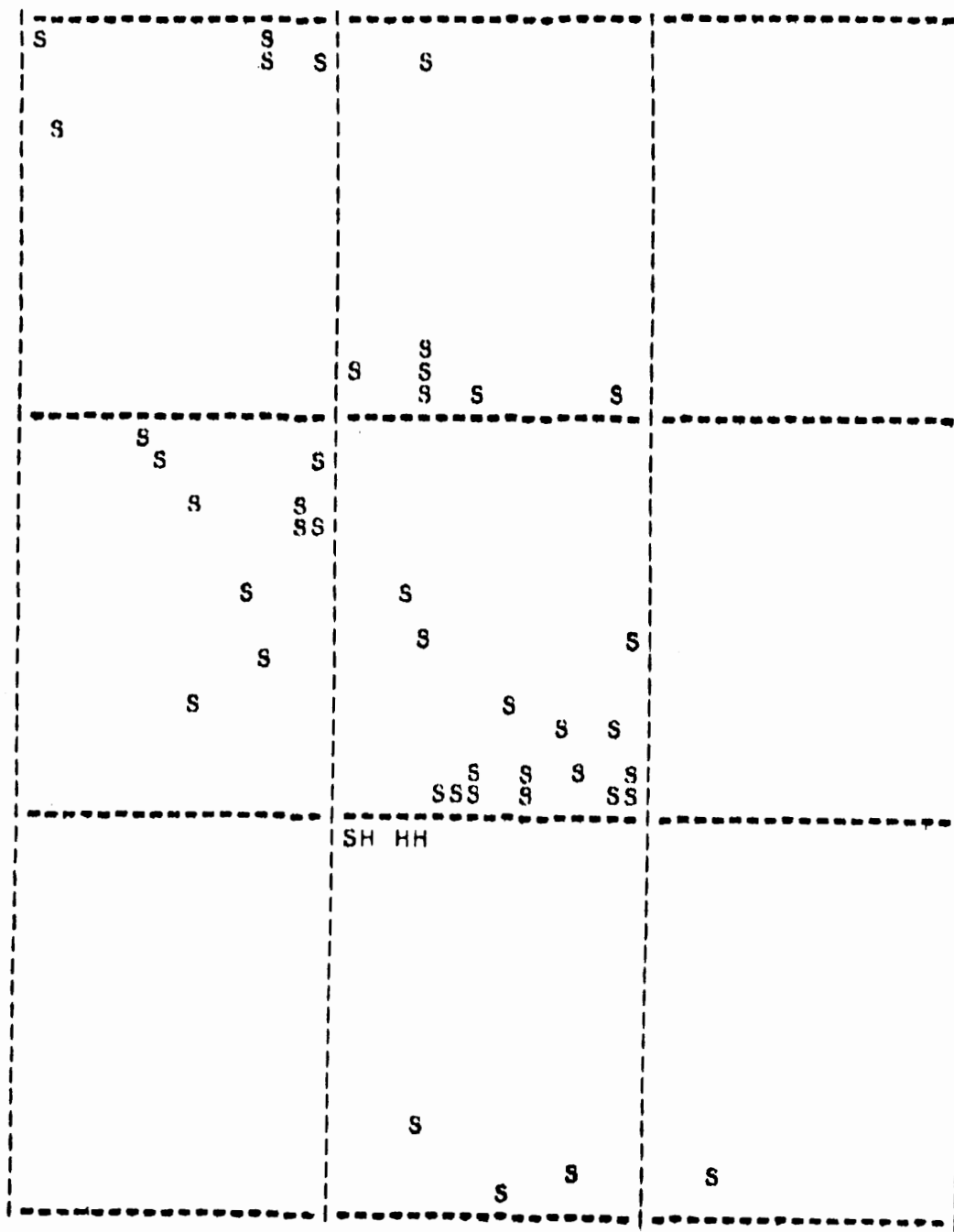
1875 Osage Land Claims- Hominy Station



Symbol is value of Station

Figure 3c.

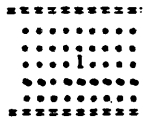
1875 Osage Land Claims-Salt Creek Station



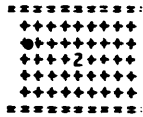
Symbol is Value of Station

Figure 3d.

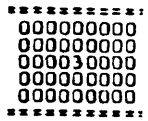
Key to Figure 4. Osage Substations.



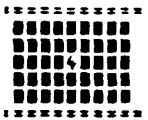
HOMINY



SALT CREEK



LITTLE OSAGE



AGENCY

OSAGE HOMESTEAD SELECTIONS
1875
PHYSICAL DIVISION

analysis (ibid: II-6). The results of a proximal map of sub agency affiliation is presented in Figure 4.

The areas defined in Figure 4 are interpreted as approximations of the dimension of Osage Physical Divisions. Reference to this map will allow the reader to locate the point maps discussed below within the reservation. The map scale required to achieve quarter-quarter section detail prevents the production of a publishable computer generated map for the entire reservation.

The next set of Figures (5a-5d) present the point maps for band affiliation. Individuals with missing values (unknown band affiliation) are represented by the character "X". Certain bands, such as Joes Band (Figure 5d) and Beaver Band (Figure 5b), display a high degree of spatial clustering, while other bands of comparable size, such as Saucy Chief Band (Figure 5b), display a pattern of heterogeneous clustering with certain other bands.

A proximal map of band affiliation is presented in Figure 6. The regions defined in this map are viewed here as approximations of the territorial extent of Osage Annuity Bands. The SYMAP proximal map routine limits the number of classification variables possible to twelve. As the Osage were divided into fifteen Annuity Bands in 1876 it was necessary to use a single character to represent more than one band. This was accomplished by combining the Tall Chief and Big Hill Bands in the Salt Creek subagency (represented by ".") and the Black Dog, William Penn and Watianka Bands in

Figure 5a. 1875 Osage Claim Selections- Little Osage Subagency

<p>H H</p> <p>H H HH</p> <p>H H</p>	<p>X H</p> <p>H H</p> <p>X HH</p> <p>X H HH</p> <p>H H</p> <p>H H</p> <p>H H</p> <p>H</p>	<p>H</p>	
	<p>II</p> <p>XX FH</p> <p>E</p> <p>X</p> <p>I H</p> <p>E</p>	<p>E</p> <p>X IE</p> <p>E I</p> <p>XH X</p> <p>E E</p> <p>XX XX</p>	<p>I</p> <p>I</p> <p>H H</p> <p>X</p> <p>E X</p>

Symbol is value of Band.

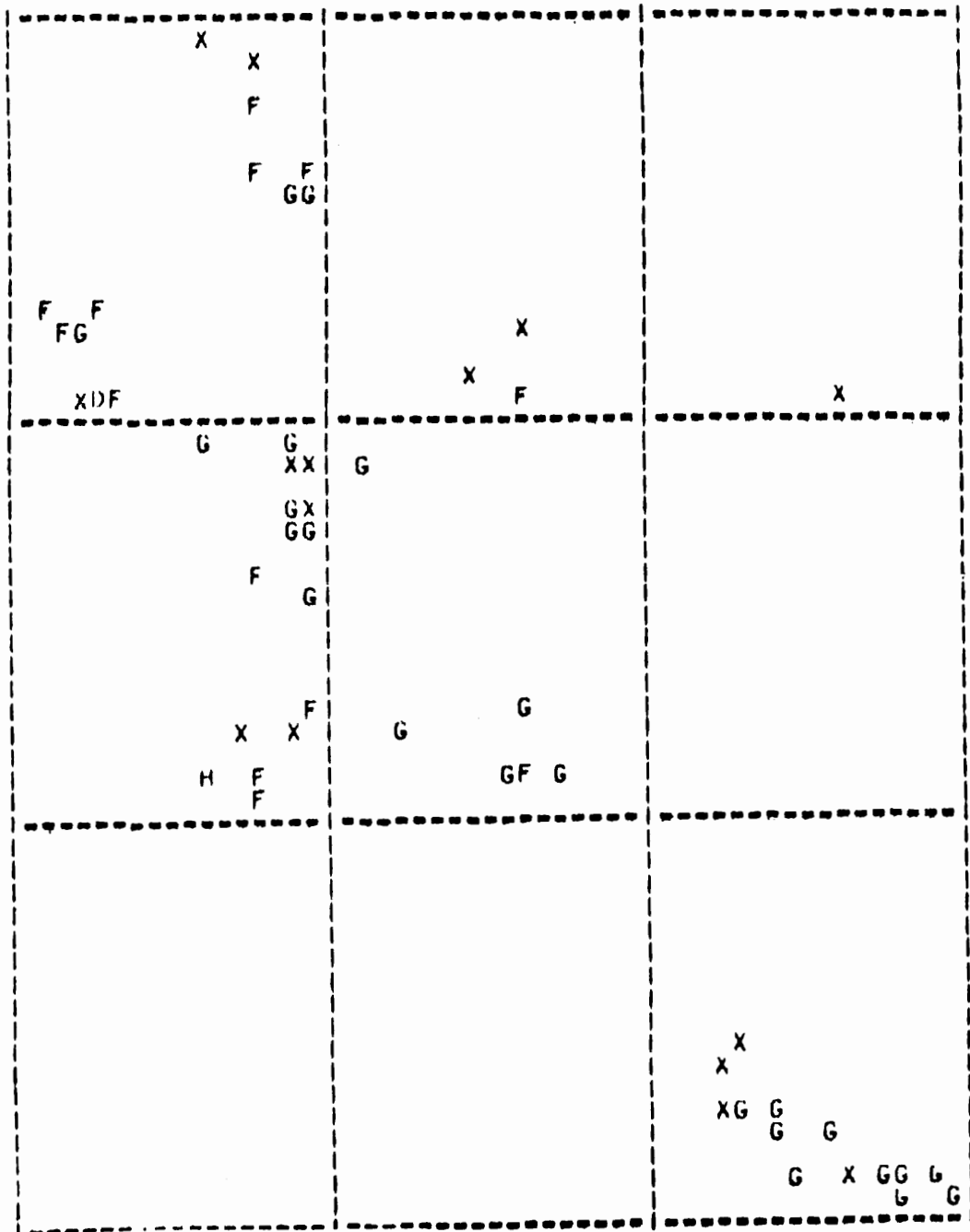
H- Mixed Blood Band

E- Strike Axe Band

I- Nonpewalla Band

X- Missing Band Value

Figure 5b. 1875 Osage Land Claims- Agency Substation



Symbol is value of Band

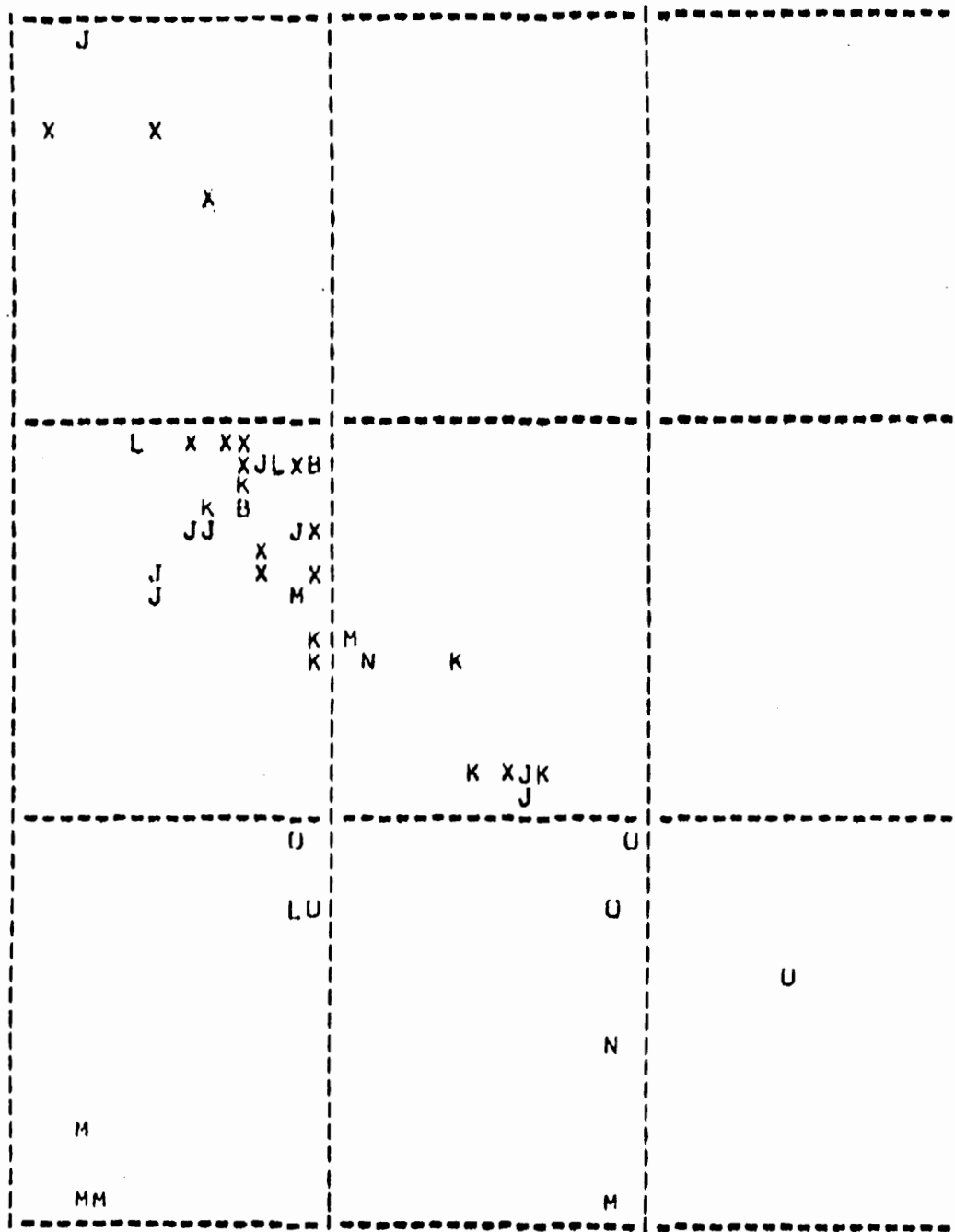
F- Saucy Chief Band

G- Beaver Band

X- Missing Band Value

Figure 5c.

1875 Osage Land Claims- Hominy Subagency



Symbol is value of Band

- J- Hominy Band
- K- Clamore Band
- L- Wm. Penn Band
- M- Big Chief Band
- N- Watianka Band
- O- Black Dog Band
- X- Missing Band Value

[illegible]

A- Joes Band
B- Big Hill Band
C- White hair Band
D- Tall Chief Band
X- Missing Band Value

KEY TO FIGURE 6. 1876 OSAGE ANNUITY BANDS

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100

JOES BAND

[illegible]

MIXED BLOOD BAND

TALL CHIEF BAND
BIG HILL BAND

H	0	0	0	0	0	0
H	0	0	0	0	0	0
H	0	0	0	0	0	0
H	0	0	0	0	0	0
H	0	0	0	7	0	0
H	0	0	0	0	0	0
H	0	0	0	0	0	0
H	0	0	0	0	0	0
H	0	0	0	0	0	0

NONPEWALLA BAND

A 10x10 grid of dots. In the center, there is a stylized number '2' formed by dots. The '2' is composed of a horizontal bar at the top, a vertical stem on the left, and a curved bottom that extends to the right. The dots are arranged in a regular pattern, with the '2' being a specific configuration of dots within the grid.

WHITE HAIR BAND

HOMINY BAND

E

STRIKE AXE BAND

BLACK DOG BAND
WILLIAM PENN BAND
WATIANKA BAND

00	01	02	03	04	05	06
07	08	09	0A	0B	0C	0D
0E	0F	10	11	12	13	14
15	16	17	18	19	1A	1B
1C	1D	1E	1F	20	21	22
23	24	25	26	27	28	29
2A	2B	2C	2D	2E	2F	30
31	32	33	34	35	36	37
38	39	3A	3B	3C	3D	3E
3F	40	41	42	43	44	45
46	47	48	49	4A	4B	4C
4D	4E	4F	50	51	52	53
54	55	56	57	58	59	5A
5B	5C	5D	5E	5F	60	61
62	63	64	65	66	67	68
69	6A	6B	6C	6D	6E	6F
70	71	72	73	74	75	76
77	78	79	7A	7B	7C	7D
7E	7F	80	81	82	83	84
85	86	87	88	89	8A	8B
8C	8D	8E	8F	90	91	92
93	94	95	96	97	98	99
9A	9B	9C	9D	9E	9F	00

SAUCY CHIEF BAND

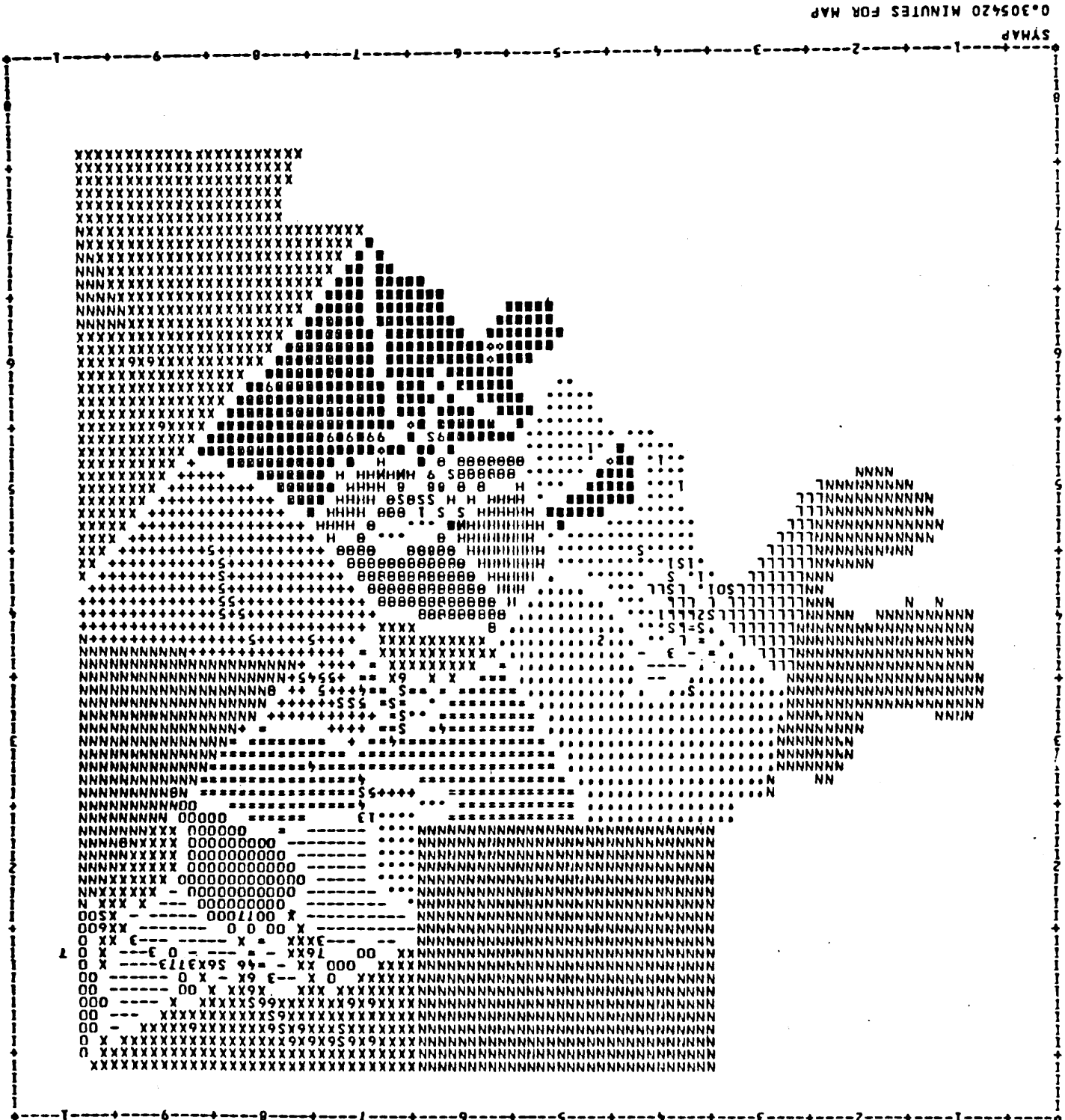
BIG CHIEF BAND

BEAVER BAND

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100
---	---	---	---	---	---	---	---	---	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	----	-----

CLAREMORE BAND

Figure 6. Proximal Map of Osage Annuity Bands



OSAGE HOMESTEAD SELECTIONS
1975
BAND DISTRIBUTION

the Hominy subagency (represented by "0"). Bands were combined based on their degree of spatial proximity to one another and sample frequency as established by through the band point maps. This combination process will obscure the distinction of these bands from each other and will present a greater degree of territorial integrity than is actually warranted. This map (Figure 6) is used in this study to provide a graphic illustration of the relationship between bands and physical divisions and not as a means of determining the number or extent of inferred band territories. Thus the distortion discussed above is not viewed as negatively affecting the conclusions drawn from this stage of data analysis.

The frequency of band and physical division affiliation of Osage male family heads is presented in tabular form in Figure 7. This table demonstrates the clear association of certain bands with particular physical divisions. The inconsistencies in data performance are quite small in number and while they might be viewed as representing the dynamic nature of band membership they do not prevent the placement of a band within a physical division. The results of this analysis provides evidence for the following chart of the band composition of physical divisions (Figure 8). Band membership can be seen to have valuable potential as a predictor of district affiliation, and thus, geographic location within the reservation.

Discussion.

Figure 7. TABLE OF ANNUITY BANDS BY SUBAGENCY

Band	Subagency				Total
	Little Osage	Agency	Hominy	Salt Creek	
Missing	14	20	15	9	58
Big Chief	0	0	7	0	7
Black Dog	0	0	5	0	5
Big Hill	0	1	2	7	10
Beaver	0	24	0	0	24
Claremore	0	0	7	0	7
Mixed-Blood	32	3	0	0	35
Hominy	0	0	10	0	10
Gov. Joes	0	0	0	14	14
Nopawalla	7	0	0	1	8
Strike Axe	8	1	0	1	10
Saucy Chief	1	14	0	1	16
Tall Chief	0	1	0	5	6
White Hair	0	0	0	6	6
William Penn	0	0	3	0	3
Watianka	0	0	2	0	2
Total	62	64	51	44	221

Figure 8. BAND COMPOSITION OF OSAGE SUBAGENCIES

Little Osage	Agency	Hominy	Salt Creek
Mixed Blood	Beaver	Big Chief	Big Hill
Strike Axe	Saucy Chief	Black Dog	Gov. Joes
Nopawalla		Claremore	Tall Chief
		Hominy	White Hair
		Wm. Penn	
		Watianka	

Several assumptions were made during the course of this study and their reiteration will serve as an introduction to the discussion of the results of data analysis. The major assumption made of the data was that the 1875 Claim selections were chosen by the Osage themselves and thus are a potential source of information regarding the local organization of the tribe. The Claim Data contained 221 selections and is viewed as a non-random sample for the tribe. Band affiliation for Osage family heads was derived from the 1876 Osage Annuity Roll and it is assumed that band membership did not change significantly between 1875 and 1876.

Computer generated point maps were used to determine the relationship between band and physical division affiliation and as a source of data regarding the local organization of the Osage in the late nineteenth century. The named physical divisions of the tribe were shown to exhibit a high degree of spatial cohesiveness with observed deviations interpreted as representing the dynamics of physical division membership. Annuity Bands were shown to exhibit two major patterns of clustering, with certain bands displaying a high degree of geographic discreteness while other bands were found in heterogeneous clusters. Areal or surface maps were produced from the point data through an application of the SYMAP proximal map routine. These maps were used to as a graphic model of the territorial dimensions of physical divisions and annuity bands and provided a quantitative foundation for the

delineation of the band composition of Osage physical divisions.

The remainder of this paper will focus on the different spatial patterns displayed by the Annuity Bands. The obvious question to ask is why certain bands exhibit discrete spatial clusters while others do not. The two forms of band clusters encountered during data analysis will be presented in this section as the geographic reflection of a difference in the subsistence strategies employed by the members of these groups. The heterogeneous band clusters are believed to represent the reformation of Osage local groups in response to a new set of material conditions and productive relations.

The 1875 claim data included the number of acres under cultivation on each claim selection and thus provides an opportunity to quantitatively examine the relationship between agricultural production and spatial organization. The Wah-xa-koh-lin Physical Division (Agency Substation) will be examined in this section as an example of the distinction between heterogeneous and homogeneous band clusters. While the conclusions drawn from this analysis may partially account for the situation in the Agency District additional analysis will be required to extend this argument to other Osage Physical Divisions.

During the initial Oklahoma Reservation period of 1872 to 1876 the Osage continued a modified aboriginal subsistence cycle which included casual horticulture and nomadic buffalo hunting. It is important to mention that involvement in

Euro-American trade relations had altered the focus of Osage hunting activities. For the purpose of the present investigation this centers on Osage participation in the buffalo robe trade. The annual production cycle of the Osages began in their permanent villages with the planting of corn, squash, and pumpkins in the spring with the young plants cared for until they were well established. The tribe would then congregate on the Plains to conduct the summer buffalo hunt. In the fall they would return to their permanent village sites to harvest their crops and prepare them for consumption while on the plains during the winter hunt. The winter hunt was of primary importance as it was the source of buffalo robes for trade (Bailey 1974: 52-53).

The correspondence and miscellaneous reports of the Osage Agency are the major source of data for a delineation of subgroups within the Osage population based on subsistence activity. The 1874 Annual Report of the Osage Agency states (AR-CIA-OA 1874:222):

As usual, about five-sixths of the tribe went to the plains in the fall, and remained there during the winter, procuring their support mainly from the buffalo. They returned in the spring with a good supply of dried meat and tallow to subsist upon until they planted their crops of corn and vegetables.

The Agent provides evidence for the fissioning of bands for differential participation in agricultural activity in his

remarks regarding those individuals who did not go to the plains to hunt buffalo (ibid):

The one-sixth that remained on the reservation embraced the mixed-bloods, about three hundred in number ... , most of three bands of full bloods who are nearly civilized and some of other bands who are civilized besides a portion of the sick and aged

These statements could be interpreted as the successful achievement of the assimilation goals of Federal Indian policy, with individuals abandoning traditional subsistence patterns to participate in row agriculture. The refutation of this stance and detail regarding the identity of the bands referred to above are gained from the following comments by the Osage Agent (AR-ClA-OA 1872:246):

Special care was taken to assist and instruct them [the Little Osage] in their attempts at farming; also the Beaver and a part of the White Hair Bands who were disposed to settle and go to work under the discouraging influence that then prevailed. Their efforts have been rewarded with a bountiful crop, and they are now feeding their less sanguine brothers who have been subsisting solely on the plains.

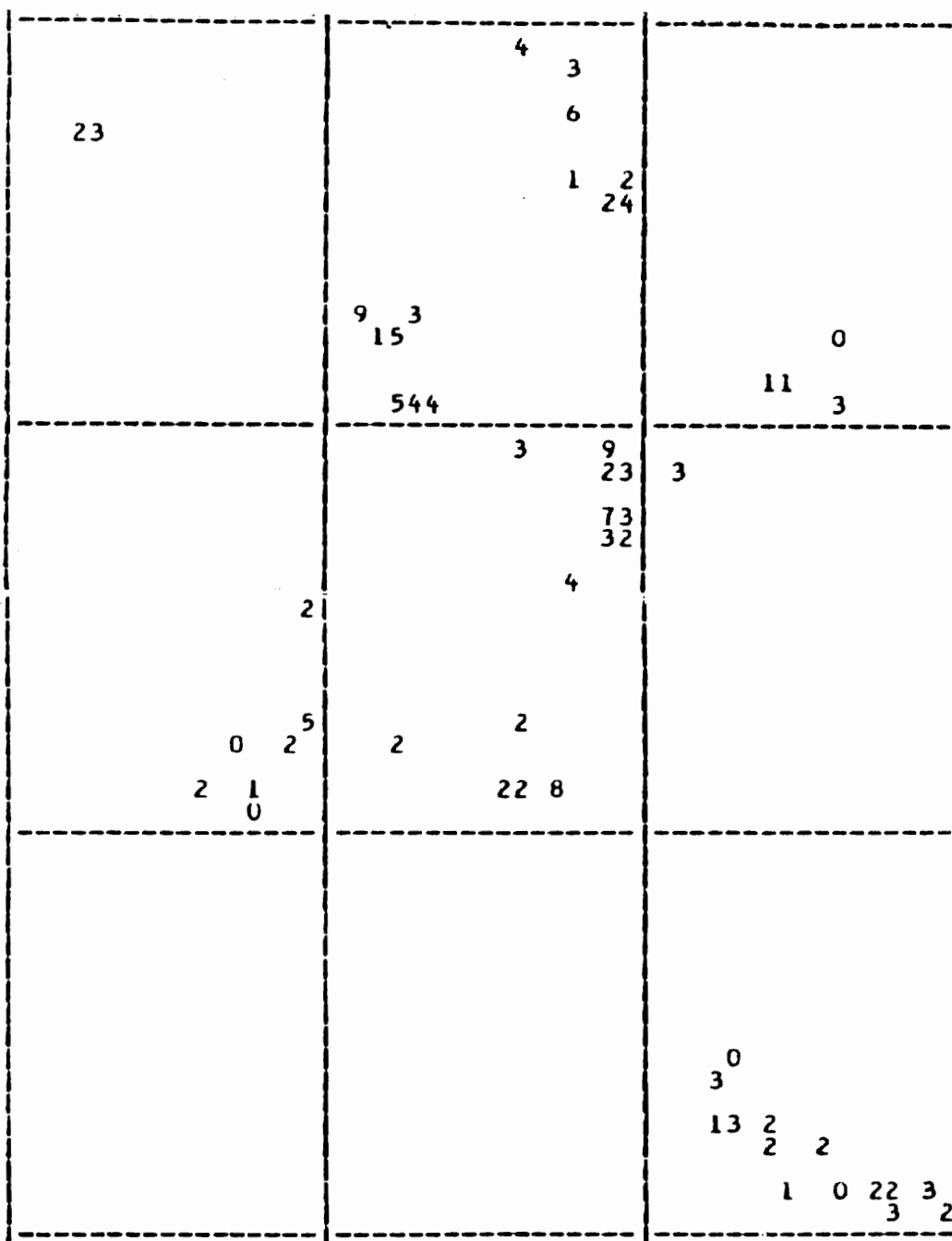
This statement provides an initial indication that subsistence strategy differentiation did not interfere with traditional patterns of resource redistribution among band members. This author believes that labor input was drawn

from the summer buffalo hunt by certain band members in order to meet the rigorous environmental demands of row agriculture in the region.

Examination of the computer map of band affiliation for the Agency Substation (Figure 9) shows that the Beaver Band was divided between two areas, one in close proximity to the agency and another on the lower drainage of Bird Creek. These divisions of the Beaver Band display the two patterns of geographic orientation discussed previously. Band members on the lower drainage of Bird Creek exhibit a well defined homogeneous cluster while those members near the Agency Station are found in a mixed cluster with members of the Saucy Chief Band.⁴ The number of acres cultivated by individuals in this district is presented in the form of a point map in Figure 10. From this map it can be seen that the members of the homogeneous cluster have lower agricultural production values than members in the heterogeneous cluster. The 10 individuals in the homogeneous cluster have an average production value of 1.9 (9.5 acres under cultivation) while the 14 individuals in the heterogeneous clusters exhibit an average value of 3.86 (19.3 acres under cultivation). This provides a clear indication for the discrimination of these geographic groups on the basis of the number of acres under cultivation.

Individuals wishing to pursue row agriculture, using Euro-American farming techniques would obviously locate in close proximity to the Osage Agency in order to obtain

Figure 10. Agricultural Production Values
Agency Substation



Symbol is number of acres under cultivation.

- 0= 0- 5 Acres cultivated
- 1= 6-10
- 2= 11-15
- 3= 16-20
- 4= 21-25
- 5= 26-30
- 6= 31-35
- 7= 36-40
- 8= 41-45
- 9= 46-50

instruction, materials and assistance. Individuals practicing traditional, casual horticulture would be more inclined to locate in ecological areas conducive to this method of farming, commonly near the headwaters of feeder streams.

The continued importance of casual horticulture should not be underestimated, however, as several instances of a poor return from row agriculture are reported. Evidence exists that "traditional patches in the timber, of from one-half to three acres, enclosed with a pole and brush fence" (AR-CIA-OA 1874:222) fared better in times of drought than did crops planted according to Euro-American practices. In 1881 the agent reported (AR-CIA-OA 1881:86):

I have visited them [the full-blood Osage] in their camps a number of times and nearly always find them at home, looking after their little bunches of stock and attending to their patches of ground. Those of them that planted early have a fair crop of corn, but late planting has proven almost an entire failure, owing to the exceedingly dry weather. Most of them have prepared their "squaw corn" for winter, and it is not uncommon to find 15 or 20 sacks carefully stowed away for future use. They raise squashes and pumpkins in large quantities.

Additional evidence for the heartiness and early maturity of Indian varieties of corn is found in the agents comment in 1874 that the drought and grasshoppers had destroyed all

crops planted "except an early Indian corn" (AR-CIA-OA 1874:223).

The heterogeneous nature of band affiliation, as demonstrated in the computer maps discussed earlier can be partially explained as a reorganization of Osage local groups based on subsistence activity specialization. A declining return from nomadic buffalo hunting for both food and robes was brought about by increased competition for a declining resource. This would place greater importance on vegetable foods and meat obtained from hunting within the confines of the reservation. This author would suggest that traditional patterns of reciprocal redistribution of strategic resources continued between members of Osage Bands. This would function to increase the number of subsistence strategies employed and maximize the probability of obtaining an adequate supply of critical resources.

Concluding Remarks.

The 1875 Claim Data was viewed in this analysis as a potential source of information for the isolation of Osage local groups and the delineation of the relationship between Osage Physical Divisions and Annuity Bands. Computer mapping techniques were utilized to determine the geographic limits of Osage Physical Divisions and Annuity Bands. The results of this analysis illustrated that Physical Divisions displayed a high degree of spatial cohesiveness and isolated two general patterns for the geographic orientation of Annuity Bands. Band affiliation was capable of partially

explaining the patterns observed through the isolation of homogeneous band clusters. The failure of band affiliation to account for the heterogeneous band clusters was viewed as the spatial representation of the reorganization of Osage Bands for subsistence strategy differentiation.

The research reported on in this paper represents a preliminary attempt to utilize quantitative data analysis to examine Osage local organization in the late nineteenth century. While a number of tentative conclusions were achieved through data analysis this effort may well have raised more questions than it has answered. If our objective is to gain an understanding of the internal adjustments which took place in American Indian societies during the reservation period of their historical experience, then we must move from a "mechanical-processual" attitude toward the "role" of change in Human Society to one which is based on a thorough understanding of the historical conditions which effect that change (Klein 1980:136).

Research strategies to achieve this goal must be sensitive to the "exceptions to the rule"; they must be capable of isolating instances when known social principles cannot account for observed social behaviors. Quantitative analysis and scientific research design should be viewed as integral components in our efforts to move beyond "cause-effect" models of social change.

End Notes

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1 The reader is directed to Allison Lee Granberry, "The expression of Osage Identity: Ethnic Unity and the In-lon-shka". Unpublished Masters Thesis, University of Tulsa, 1987.

2 Additional technical assistance was received from Richard Satler.

3 For a discussion of land selection patterns in the 1906 allotment of the Osage reservation see, Susan C. Vehik, "The Osage Allotment: A Preliminary Analysis of Land Selection Patterns", *Papers in Anthropology* 21 (2):93-106, Norman.

4 The Beaver Band was also divided between the Agency and Bird Creek Reservation Districts. The Bird Creek District has been previously identified in this paper as the Non-dse-waspe, or "Heart Stays" Physical Division of the Osage tribe. It's identity as such has been obscured in the 1875 Claim Data as there was not an agency substation established in the Bird Creek district. The heterogeneous cluster of members of the Beaver and Sancy Chief Bands can be viewed as the initial stage of the incorporation of the Non-dse-waspe into the Wah-xa-koh-lin Physical Division.

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Toward a Better Approach to American Indian History

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Newberry Library

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I try to approach the study of the past in the same way I approach the study of the present, from the perspective of cultural anthropology and as a "participant-observer" fieldworker. One book on fieldwork that I particularly admire is Doing Fieldwork by Rosalie Wax. She describes fieldwork very well: borrowing a phrase from Marx--Groucho, not Karl--good fieldwork is the willingness to "wrestle anyone in the house." What she means is that we fieldworkers must pursue every lead, try every approach, to reach an understanding of what we observe. That, in my opinion, is what good historical anthropology is--looking for every type of document or record, approaching each question from several directions. We should, then, search out and critically study data such as census and allotment records and household surveys. We should just as diligently look for the kinds of data that help us make sense of enumerations and correlations.

Some questions can best be answered quantitatively; statistical

analysis can complement, support, or cast doubt on other interpretations of social structure and change. But quantitative methods should not replace other techniques, and findings based on quantitative methods should themselves be subjected to critical review based on what people say about themselves, what they are observed to do, and what kinds of relationships they have with the wider society. "Numbers"--census and survey tabulations, for example--taken out of cultural context can be misinterpreted and can mislead rather than enlighten.

To explore this point further, "census" or enrollment rolls often include individuals' "blood degree"; yet, to take this data at face value, to use it to compute degrees of assimilation to the wider society, would be misleading. On Wind River and Fort Belknap reservations, for example, these blood degrees were assigned quite arbitrarily. Agents listed most Wind River Arapahoes as "full blood" in the late nineteenth century. A few individuals who were known to be non-Indians--these were children who were captives adopted by Arapahoe families--were listed as "one-half." The Gros Ventres' agent at Fort Belknap listed virtually all the Gros Ventres as "full-bloods" even though for a century before they settled on the reservation French-Canadian, English, and American traders had fathered children by Gros Ventre women. These children were reared by their Gros Ventre

relatives and were culturally Gros Ventre. By the twentieth century--when for various reasons federal officials were intent on reducing the number of "Indians"--children who had a non-Indian parent were enrolled as "one-half Gros Ventre blood." Thus, one could not conclude merely by comparing the rolls of 1896 and 1934 that there were genetic differences between the populations at these two points in time. And certainly "mixed-blood" or "half-blood" Gros Ventres were not necessarily any less culturally Gros Ventre or less identified as Gros Ventre than those on the roll as "full-bloods." The point is that one cannot rely on Indian Census records to discover demographic or other kinds of trends. One must also make the effort to learn individual and family histories, to understand native ideas about identity, and to place these and other data in cultural and historical context.

Names must be viewed in cultural context as well. As Neils Braroe has shown, Plains Crees are known by English names; they conceal their Indian names from outsiders, hide their "Indianness." At Wind River, the agent recorded English translations of Arapahoe names of adults but recorded native Shoshone names--not because Arapahoes and Shoshones requested him to do so, and not because Arapahoes were more "acculturated" than Shoshones, but because translations of Shoshone names often struck the agent as offensive. Again, cultural and

historical context is all-important. When Arapahoe children went to boarding school, school personnel gave them English names--Dewey, Vanderbilt, Washington, Lincoln. But these children also had Arapahoe names and not infrequently were given new Arapahoe names for religious reasons, all unbeknownst to the census taker. One could not safely draw conclusions about school childrens' and their parents' relative orientations to white society merely by comparing their names, by correlating degree of "acculturation" with degree of anglicization of the names.

The organization of data by age cohorts is also risky if done out of cultural and historical context. It is certainly valuable to compare age groups over time but chronological age may not be particularly revealing in itself. In trying to make sense of sociocultural change at Fort Belknap, I found that what was most illuminating was the identification of Gros Ventre "generations"--in the words of Karl Mannheim, a group of peers born within a particular time span whose shared experiences significantly distinguish them from other age groups. This done, I compared the generations' ideas and behavior and looked at how interaction between the generations influenced change. The generation born between 1895 and 1929 had some shared life experiences that shaped their perspectives and choices. Briefly, they were familiar

with Gros Ventre ceremonial and political life in its heyday, yet were not encouraged by their elders to assume these ritual and political roles. The prestige of the Gros Ventre business council was on the wane and Gros Ventre ritual life had declined by the late 1930s. The generation born between 1930 and 1955 had not experienced firsthand the ritual and political institutions that their elders (the 1895-1929 generation) viewed as "traditional." Yet this younger generation were the major participants in the War on Poverty and affirmative action programs of the late 1960s and 1970s. Many attended college and there further developed their ideas about Indian identity. Many became involved with Native American activist organizations and returned to Fort Belknap determined to further the cause of self-determination. This generation was committed to "reviving" Gros Ventre ceremonies and reorganizing the business council. The course of change at Fort Belknap during the 1970s was set by the interplay of ideas--interpretations of the past, reaction to contemporary events--and the social interaction between the 1895-1929 and 1930-1955 generations. To give one example, the youths emphasized the revitalization of native religious rituals, while the elders opposed them. As a result the cultural revival of the 1970s focused on secular ritual, initiated and led by youths.

In the early twentieth century at Fort Belknap, a similar process

was at work between the generation that came to maturity prior to reservation settlement and the generation born on the reservation in the late nineteenth century. The early twentieth century--like the 1970s--was a period of major political and ritual revitalization among the Gros Ventres. The older Gros Ventres had many among them who had supernatural powers in war and curing, who had amassed an impressive war record, and who held high positions as ritual authorities. But for the younger Gros Ventres born too late to have apprenticed to acquire supernatural power, to go to war, or to be initiated into the sacred ceremonial organization, the urge for prestige and for a ritual life meaningful in the reservation era led to the development of new ceremonies. Elderly people were incorporated in various phases of the newly reorganized Grass Dance, for example, but these new Grass Dances were shaped by young Gros Ventre men and women to give expression to contemporary goals and relationships. While it may be enlightening to know, then, how many sixty-five-year-old Gros Ventres there were in 1901 or what percentage of these people had children, it may be equally or even more important to know what it meant to be in one's sixties or in one's twenties at a particular point in time. How did the life experiences of people in their sixties or in their twenties shape the choices they made and the influence they had on their society?

Knowledge of the cultural and historical context of age, or any other aspect of life, is central to our interpretation of the past or of change, as it also is for our ethnological studies.

My intent here is not to demean the contributions of the participants in this conference; I found their papers informative and provocative in suggesting the kinds of sources that might provide useful data and in suggesting interesting hypotheses about social change. What I have tried to emphasize is that, in interpreting the past, scholars should not, in fact, cannot, eliminate qualitative analysis. But few scholars would take so extreme a position. I do, however, have reservations about the conference's focus on a "quantitative approach to American Indian history."

In Indian history there is a need, not so much for an emphasis on quantitative methodology, as for revamping some analytical concepts that have become traditional catch-phrases in the field of Indian history and that have turned up again and again in our discussion over the past two days. In fact, I am going to argue that we would do well to eliminate them from our work altogether. They are the following: acculturation, factionalism, pan-Indianism, progressive, mixed-blood, and I suspect many others (alcoholism, perhaps). Our objective should be to describe what happens over time and to account for it. The use of the preceding

terms obscures these goals. I have time today to discuss some of the problems surrounding the terms "acculturation" and "factionalism."

Individuals, groups, and whole societies have been described as "acculturated." Comparisons are drawn between less and more acculturated groups or societies. But what does the term really mean? What social and cultural facts does it describe? And can the term be used cross-culturally? Let me approach these questions by looking at two Plains societies, the Fort Belknap Gros Ventres and the Wind River Arapahoes.

One history of the Gros Ventres describes them as acculturating when they ceased to hunt and to hold men's society dances and began an agricultural way of life. As agriculturalists they held an elaborate fair with rodeo and other contests and judging of agricultural produce. Yet, ranching for Gros Ventre families in the late nineteenth and early twentieth centuries was a means to an old end--accumulating surplus stock to distribute generously to others, which bolstered a man's (and his family's) reputation as prominent. Wealth in cattle as well as wealth in horses served as a mark of status and authority. Then men's societies, which were part of a moiety system, were replaced by residence-based moieties that competed in contests of generosity just as the men's moieties once did. The residence-based moieties were

introduced by an elderly Gros Ventre medicine man who visited Crow Agency and saw the Crow Fair. He returned to Fort Belknap, started the Gros Ventre fair, and initiated the reorganization of Gros Ventre society into the Black Lodge and Mountain Crow--I learned at this conference that these names refer to competitive Crow tribal divisions. The highlights of the agricultural fair, organized by the moieties, were sham battles and other contests between the moieties. The display of agricultural produce was secondary to the reaffirmation of Gros Ventre values and the representation of an old pattern of social organization in an innovative ritual context. Thus, the fair and the Gros Ventres' commitment to stockraising was a means to adapt to reservation conditions in ways that made sense to them as Gros Ventres. It was in no way evidence of their rejection of Gros Ventre identity or values.

The Gros Ventres also sought allotments in the early twentieth century. This fact led some scholars to label them "progressive" and to describe their leaders as young "mixed-bloods." But the request for allotment was in reality a strategy on the part of virtually all the adult Gros Ventres to prevent further land cessions and for tribal leaders to gain greater control over reservation resources. These leaders were both young and old and were culturally Gros Ventre. Yet their request for allotment was characterized as evidence of their

acculturation to white-American values.

Arapahoes were studied in the late 1930s. All but the elderly were portrayed as acculturated to varying degrees. Young men were described as alienated from their Indian heritage and identity because they were not prominent in political and ritual affairs. But in point of fact extensive participation in this society with its age grade tradition was appropriate only for the elderly. Years later these same "alienated" young men were the political and ritual leaders of their day.

Moreover, to describe both the Gros Ventres and Arapahoes in the 1930s as "acculturated" is meaningless. These peoples' orientations and outlooks and their social organizations, although different from those in their pre-reservation past, were quite distinct. Even today studies of Indian history often describe social innovation or behavior without giving attention to the meaning of those innovations or behaviors to native peoples themselves. All too often acceptance of white-American ways of thinking and doing things is assumed to be inevitable, so scholars simply do not make a thorough inquiry into new institutions that seem, on the surface of it, non-Indian in origin. Falling back on the old standby, the acculturation continuum model, hinders our ability to compare and contrast peoples in insightful ways.

Let me turn now to the use of the term "factionalism." Descriptive

of groups or societies in this way not only helps to obscure political process but may erroneously suggest that groups with different perspectives do not influence each other and thereby contribute to innovations that may prove adaptive. Viewed over time, conflict, we know from anthropological studies elsewhere, can contribute to adaptive social reorganization, to new ideas that help revitalize a society, or to reinforcement of alliances. But the "factionalism" label, as it has been used in Native American studies, suggests a lack of cooperation generally, when in some cases--as at Fort Belknap--people who vehemently disagree over political strategies and the meanings of ritual symbols cooperate regularly to mobilize support for reservation ventures and to jointly participate in the very rituals about whose symbols they disagree.

At Fort Belknap, Gros Ventres identify a "militant" and an "educated" faction among "youths," people in their thirties and forties. These groups have different interpretations of particular historical events and of certain ritual symbols, and they may oppose each other politically in questions about how treaty claim money should be spent, for example. But they help each other in giveaways, jointly participate in powwows, and cooperate in organizing community celebrations or activities. They may be members of the same family who relate smoothly

in the context of household activities. Their contrary viewpoints have not isolated them from each other nor caused a breakdown in social cooperation. Yet if these social groups were labeled "factions" and political process described as "factionalized," that is the impression that would be conveyed. Actually these kinds of conflicting perspectives and contested meanings, in the past as well as today, have led to symbolic reformulations and social reorganization at Fort Belknap that have helped the Gros Ventres successfully adapt to new social and historical contexts. On other reservations, as well, groups in conflict affect each other's perceptions and choices; from this process often comes major sociocultural change, not necessarily paralysis or chaos.

A better approach to American Indian history would be to try to focus on how Indians viewed the world, how they behaved toward other people and each other, what their political goals, values, strategies and options were--without assigning them in the process to acculturation stages and without reducing their political conflicts and problems to "factionalism". As students of American Indian history we should describe what we observe without forcing these observations into traditional models that have contributed not only to negative stereotypes about Native Americans but also to misunderstandings about change.

Reflections on Quantitative American Indian History and
the Pueblos of New Mexico

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I would like to comment on several of the conference papers and then make some observations on quantitative history in my own specialization, the Pueblo Indians of New Mexico after 1846. My remarks are random, not intended as balanced reviews but rather as reflections on certain points of concern.

In his keynote address Professor Morris remarked on the need for clear thinking. It is an admonition we should always bear in mind. Unexamined assumptions compromise the value of research findings, whether quantitative or not. In his innovative study of Comanche trade and political organization, Tom Kavanaugh has not adequately examined his assumptions about the relationship between economic and political forces, whether of Indian or white society. Although he admits that Harold Lasswell's famous aphorism that politics means "who gets what, when and how" is simplistic, his paper is predicated on that very definition. (Incidentally, that aphorism was coined in the 1930s, a decade preoccupied with political response to economic catastrophe. Laswell's book, Politics: Who Gets What, When, How, was first published in 1936. Despite the subtitle, it is not a work in economic determinism, but in the many-faceted sources of political influence, including personality, symbolism, propaganda, and violence.) It may be true that Comanche political leadership was based upon the ability to deliver goods. Kavanaugh makes a good case for a strong connection. But he cannot prove it the sole factor with an a priori claim that economic power controls politics. Of course, the two are linked; every society that organizes itself politically must also have means for producing and distributing goods. But politics is not just a function of economics. It has its own sphere and vitality,

including non-economic determinants, among them the manifestation of nationhood, the expression of social and ethical values, the display and renewal of national myths and symbols, public order and safety, the drive for personal achievement, and the promise of a collective life after the death of an individual. Economic factors do not adequately explain such political phenomena as McCarthyism, immigration restriction, the "right to life" movement, Indian reform legislation, the selection of traditional Iroquois chiefs, or the "power domain" of Pueblo Indian caciques. Comanche chiefs must have been successful brokers of trade goods, gifts, and war booty, but that alone cannot adequately account for their status.

Daniel Swan's paper on the Osage presents a different kind of problem. His primary interest seems to be in the application of computer technology to a large data set, namely the 1875 land claim schedule of Osage family heads, after which he offers agricultural, band-affiliation, and peyote-cult analyses of what he has found. The organization of his paper suggests that he finds the computer more engrossing than the issues he intends to explore. If I were a specialist in Osage history, or a computer buff, this might not bother me, but as it is, I do not know enough about Osage agriculture, band affiliation, or peyotism to appreciate what difference their geographic distributions within the Osage reservation really make. Swan did not set out to test an hypothesis so much as he did to test an instrument. His data are imposing, his maps are impressive, but unless the world of Osage experts is larger than I think, he had better give more attention to the thirst of friendly ignoramuses like myself. In papers of this ilk, the temptation is to let the medium dominate the message. The computer is so attractive for organizing and enhancing statistics that we become preoccupied with its elegance, or at least its power, and wind up admiring the tool more than the questions. In his conclusion Swan says that one must understand "historical conditions" in order to understand internal reservation changes. I should have thought that point obvious,

and not in need of demonstration through "format configurations of the Statistical Analysis System."

To surround ourselves with statistical information is to risk imprisonment by it. The statistics then dictate our horizons. I note that there are at this conference no papers on the eastern tribes in colonial times, and little attention, save in Kavanaugh's paper, to Indian affairs before the reservation era. This is surely not a coincidence. Statistical data on Indian affairs before the Civil War is thin compared with the late 19th and early 20th centuries, when a bureaucracy emerged that collected figures on pacified, stabilized tribes. As we turn to the reservation era, which has been so neglected until recently, let us not ignore the earlier history in turn.

Statistics may, of course, be a great asset in dealing with larger questions. Melissa Meyer's paper on the Chippewa at White Earth reservation provides historical, ecological, and cultural commentary on the reservation's segmented settlement. Statistical analysis reinforces her assessment of significant band differences. As Professor Meyer says, "research issues and questions should direct quantitative research." This does not mean that all the questions must precede research; frequently we do not know enough to ask the right questions before we examine the data. But it is our responsibility to look for good questions, and--as she is well aware--to organize our findings accordingly.

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On November 8, 1880, the Pueblo Indian Agent at Santa Fe, New Mexico, Benjamin M. Thomas, received telegraphic instructions from the Superintendent of the Census to take an enumeration of the Pueblos and submit it in the next three weeks. (B. M. Thomas to Commissioner E. M. Marble, December 23, 1880, Pueblo Agency files, Entry 20, National Archives, Denver Branch.) Thomas at that time was already overworked with problems of stock theft, school construction, land titles, railroad encroachment, inadequate clerical

help, and so on, but even had he been basking in the sun, the request was impossible. There were 19 Pueblos in New Mexico, scattered over 350 miles from Taos to Zuni, each one of which would have to be visited and the members of each family listed in order to make a proper enumeration. No one had been assigned to do this work under his direction. Thomas complained that the task involved knowing five or six languages, and that the information sought was so elaborate that it "could not be procured in the most intelligent community in the United States." (Ibid.) But he was a resourceful man. He shortly reported that the population of the Pueblo Indians of New Mexico was 9,534, and then set about to "employ the necessary assistants to proceed with the work." (Ibid.)

Agent Thomas was a scrupulous man. The population figure that he submitted was not off the wall. He had been Pueblo Agent since 1874, knew every one of the Pueblos first-hand, and talked frequently with their officers. He knew the previous census data, and had a good idea about the increase or decrease of Pueblo village populations since then. What he submitted to the Superintendent of the Census was not an enumeration but a calculation. Our mistake would be in viewing the number 9,534 as an enumeration. But it would be equally a mistake to view the number as a mere guess. As Thomas said, he had complied with the Superintendent's orders "as nearly as possible." (Ibid.)

The necessity for care in the use of historical Pueblo data may be further illustrated in the agricultural statistics that Pueblo agents gathered and reported to the Commissioner, who in turn published them in the Annual Reports. Take, for example, the Pueblo agricultural data from the Annual Report of 1873. The data seem to have been collected with some care. The Indians owned 2,236 horses, cultivated 13,940 acres, and raised 10,215 bushels of wheat. But seemingly they raised no corn. At any rate, the space for reporting their corn production is marked with dashes, which means either that there was none, or that the Agent did not have time to find out, or that he asked the Indians

and they ~~would~~ ^{did} not tell him, or that he had an approximate idea but for some reason wasn't giving it, even though he was clearly making estimates elsewhere, such as in his statement that the Indians broke 2,000 acres of land for farming that year. Why no data on corn production? Other Indian agents in the southwest reported corn production for other tribes, such as the Apaches at San Carlos, and the Commissioner reported a total of 1,863,000 bushels of corn raised by American Indians that year; but not an ear was counted from the corn fields of the Rio Grande. My guess is that the Pueblos were able to tell the Agent how much wheat they had raised because they sold enough of it to consider it a cash crop, and therefore something to be measured; whereas corn was a sacred crop, the gift of the corn mothers and the shiwanna (Keres pueblos), no more to be measured than one measures the blood in one's veins; it was consumed domestically, with thanksgiving. The Pueblos could not have told the Agent how many bushels of corn they raised had they wanted to. They could and probably did tell the Agent whether it had been a good year for corn, but that is not information that fits into statistical tables. The one absolutely untenable conclusion is that the Pueblo Indians did not raise any corn. Needless to say, it follows that the Commissioner's statistic for total American Indian corn production that year is flawed.

A number of years ago an article appeared in Harper's Magazine entitled "There are 00 Trees in Russia." (Harper's, October 1964 /v. 229/, 59-65.) The essay concerned the manner in which news articles were written for Time Magazine, and more broadly, about the American fetish of quantitative minutiae. A Time staff writer having a point to make might not have all the data at hand, and rather than look them up, would type "00" in his draft and send it to a library researcher to fill in the number. Sometimes no data existed. This was the case for a statement on the number of trees in Russia. No one had ever counted them, and it was inconvenient just then to send the researcher abroad to undertake their enumeration. She did the only thing reasonable, short of quitting: by multiplying the approximate number of square miles of

forest in Russia, according to the Soviet government, by the number of trees that are supposed to grow in an average square mile, she reported a statistic, which, once published in Time Magazine, became a fact. ("Fact": Latin, "factum," something made.) Why did Time Magazine pretend this way? Because it is the quintessential American news magazine, and Americans worship statistics: we assemble them and plant them out like prayer sticks, in the belief that they will bring us prosperity and good fortune, or fame, or at least the respect of myriad other Americans who spend their lives filling in spreadsheets, scorecards, grade-books, scholarly journals, and other fetishes. I am not saying that historians should scorn statistics, but rather that they should not venerate them. Historical statistics are not, despite their appearance, a special category of evidence, somehow more objective, more reliable, more revealing of the past as it actually happened than narrative information can ever be. Statistics are simply one more form of grist, to be milled with the same circumspection that one would treat any other kind of primary historical evidence. If so handled, they can greatly enrich our understanding of Indian history. The circumspection is mandatory. By way of example, I shall turn to some statistical data from the 1890s on Pueblo education in government Indian schools.

The Santa Fe Indian School, founded in 1890, had in the fall of 1898 an enrollemt of 252 pupils. (A.H. Viets to Commissioner, November 3, 1898, Santa Fe Indian School, Miscellaneous Letters Sent, Pueblo Records, Entry 32, National Archives, Denver Branch.) An incomplete student roster for that academic year, which was probably made in January, 1899, listed 177 pupils, with a median age of 11½. Of these 177, 145 were Pueblo Indian. The majority of the 177 were listed as having enrolled in 1898. Of the remaining 77, 20 were enrolled in 1897, 16 in 1896, 33 in 1895 and 3 in 1894, no pupils who had enrolled enrolled between 1890 and 1893 were still at the school. The other 5 are unidentified by date. The implications of these

facts is that there was an enormous turnover of pupils at this government boarding school, and that after 8 years of continuous operation, the average duration of attendance was less than two years. And if the turnover was as rapid as that, one might readily understand why literacy at Indian schools such as this one proceeded at a distressingly slow pace.

On closer examination, however, it turns out that some of the pupils who were listed as having enrolled in 1898 were in fact on vacation from the Santa Fe Indian School the preceeding August, and therefore cannot have been first-year students: they were at least second-year students. For example, all the pupils from Taos Pueblo are listed as having entered the school in late 1898-- 6 on October 26 and 1 on December 7--but four of the seven, namely, Antonio Romero, Bernal Lujan, Manuel Cordova, and Julian Sousa, were listed elsewhere as being at home on vacation the preceeding August. (Superintendent's memo, August 12, 1898, same file.) There were, in fact, no fewer than 57 Pueblo pupils home on vacation at that time. (Ibid.) Why should the Superintendent have listed any of them as new pupils that fall? Because it was evidently his policy to treat pupils who did not return from summer vacation on time, in September, as new students. There was a rule in the Indian boarding schools that entering pupils had to enroll for a minimum of three years, during which time they were not supposed to go home, and the boarding schools provided them with no funds for transportation home in the summertime. Most of the Pueblos at the Santa Fe and Albuquerque Indian boarding schools, however, were close enough to their villages that this rule did not stop them. Pueblo students who wanted to go home did so, and Superintendent A. H. Viets had to make the best of it. So he granted summer vacation leave with the admonition to the students, their parents, and the Pueblo governors that the pupils should return when school resumed. Frequently they did not. When they came back a month or so late, Viets started the meter running on their enlistment all over again, thereby disciplining the late returnees, giving them the opportunity to extend

their years of boarding-school education, and misleading the unwary historian.

The incidence of late enrollment each fall was remarkably high. Of the 100 pupils whom Superintendent Viets listed as enrolling in 1898, only 33 enrolled in September, the month that classes began. Sixty-three pupils came in October, 2 in November, and 1 in December, leaving one pupil who had enrolled the preceeding February. (Student roster /January? 1899/, Santa Fe Indian School, Miscellaneous Letters Sent, v. 1, Pueblo Records, Entry 32, National Archives, Denver Branch.) Clearly, October was the most popular month to go to boarding school. The reason for this is easy to see: school children were needed for harvesting the crops. (J. B. Grozier to Pueblo Agent, September 30, 1899, Pueblo & Jicarilla Agency, Letters Received, Microfilm M 1304, Roll 29.) The Indian Office had a way of ignoring this fact and beginning the school year for Indians the same month that urban schools for white children began, in September, which makes sense given the premise that Indian schools were supposed to ape white society. Pueblo families must have seen this, and most of them declined to cooperate at the expense of their crops. The fact that so few pupils came in September must not, however, overshadow the fact that almost none waited until November or December. These truants were not dragging their feet. They returned to school as soon as they could. Their families simply could not afford for them to return in September.

There is virtually no limit to the large questions that curiosity may evoke from seemingly trivial statistics. I have recently been comparing government property reports for 1898 from the teachers at various Pueblo Day Schools to see what textbooks these schools had on hand. One conclusion is hardly surprising: the vast majority of books were English readers. In the 13 reporting schools there was a cumulative total of 444 readers. Ten of the 13 schools had a dictionary. Five of them had geography books and five had history books, commonly half a dozen copies of either one.

Only 7 of the 13 schools had arithmetic books. Perhaps the teachers in the other six schools were teaching arithmetic by rote, chart and blackboard, but there was clearly no common policy about the use of arithmetic books. What surprised me is that every one of these schools had a copy--a single copy, obviously intended for the teacher--of something called Songs of Nations. Classroom music was an everyday activity. The pupils of Acoma Day School, for example, sang or attempted to sing popular songs of American culture four times a day. (Cora A. Taylor, Acoma Day School Program, February 11, 1899, Pueblo & Jicarilla Agency, Letters Received, M 1304, Roll 29.)

Evidently the Indian Office considered it more important for the teachers of Indian children to have a song-book than to have a dictionary or textbooks in arithmetic, geography, or history. Have we missed something here? Historians tend to know little about the role of music in the socialization or acculturation of Indian children. But if Europe in our own time has been Americanized by jazz, may it not be that Indian children 90 years ago were more influenced by the rote learning of American songs in an unfamiliar language than they were by the formal study of numbers or the printed page? Indian children in the boarding schools learned readily how to play brass instruments; the Santa Fe and Albuquerque Indian School bands gave popular Sunday afternoon concerts to white audiences, refuting social Darwinism harmonically; and if day-school pupils were singing "Frere Jacques," "Good-by My Lady Love" and "Auld Lang Syne," it may have counted for something. I suggest that, like the classroom clock, American songs helped to accustom Indian children to the regimented nature of American society: for the 8-tone-scale, set-key, single-mode, fixed-meter, standardized-harmonic songs of 19th century Euro-American culture are highly regimented by comparison with the music of Indian tribes. Beyond that, who knows? perhaps children found amusement in singing bizarre American songs and learning what the words meant. If all this sounds like a far cry from quantitative history, we should nonetheless listen for the echo.

"Towards a Quantitative Approach to American Indian History" is exactly the kind of conference the D'Arcy McNickle Center should be sponsoring. It reflects, of course, the concern of Fred Hoxie and his colleagues about the place of American Indian history within the field of American history. And it is not the first time that attention has been paid to this matter. Perhaps the most widely read piece on this subject would be Reginald Horsman's review essay, "Well-Trodden Paths and Fresh Byways: Recent Writings on Native American History," (Reviews in American History 10:234-44) published in 1982. Horsman spoke pointedly to the extent to which students of Indian history had not been affected by significant developments in the writing of United States history. Quantification certainly represents a case in point.

I come to this conference, I suppose, as the identifiable Luddite. I have not used quantification in my work and I am not strongly inclined to change the way I do business. Yet I recognize that this may be a useful tool for us and I believe it behooves us to think about the advantages it may offer. Ultimately, it seems to me, one must ponder the questions we want to ask, the answers we hope to obtain, and the audience we seek.

History is classified as a field in the humanities and as a discipline in the social sciences. I think it is for the better that it maintains such dual citizenship. People who study Indian history

surely may profit from methods and insights gained from the social sciences. One may look to the work of Richard White, Loretta Fowler, and other scholars to see some impressive examples. At the same time, history's ties to literature, philosophy, and other areas of the humanities have also been drawn upon by many scholars exploring the Indian past.

Not all social scientists are comfortable with that legacy. In his contribution to The American Indian and the Problem of History, Henry Dobyns spoke disparagingly of what he termed "the 'Ah, the wonder of it!' approach to cross-cultural experiences," which in his view led only "down analytical dead-end streets." "It signifies," he added, "an individual's retreat into introspection when faced with significant stimulation. In historical analysis all that such a retreat into one's self achieves is to return the level of interpretation of events to the zero-sum of each individual's consciousness." (Henry F. Dobyns, "Demographics of Native American History," in Calvin Martin, editor, The American Indian and the Problem of History, Oxford University Press, 1987, pages 67-68)

Dobyns is interested in demography and is unhappy, to put it mildly, with approaches to his subject that are not properly rigorous. His denouncement of the "literary tradition" seems worthy of extended quotation:

If the history of Native American interactions with invaders is to be anything more than chronicle written by authors with

literary pretensions, it must become a social science. To do so, historical analysis must accumulate understandings that accord with interpretive theories of anthropology, sociology, economics, and demography. Scientific history cannot, like literary history, collapse into individual, atomistic introspection when confronted with data. Nor can history become a social science long as useful analytical concepts collapse every time a writer pens another passage of precious prose. All too many historians pay homage to their literary tradition by striving for elegance of expression at the price of accuracy in both analysis and communication to others. Frequently, the cost of the felicitous phrase is the loss of replicability of analysis--a requirement in a science. (Ibid., page 68)

Now one wishes that Dobyns would not be so restrained in his commentary, but his stern sermon would be echoed by many other observers. His admonition underscores some of the issues inherent in debate over quantification. To what extent are Indian historians only interested in the individual, the particular, the unique? Are situations sufficiently different within Indian communities that broader portraits cannot be drawn? What kind of data do we seek and how do we use it?

Fred Hoxie, Melissa Meyer, I, and others agree that the period from the 1870s through the 1920s is an era that demands greater attention from historians. Hoxie and Meyer are attempting to use federal census data and other information to give us a more complete

picture of Indian communities from this critical transitional time in American history. I will be very intersted to see their findings and to muse over the interpretations. But I will also be concerned about the kind of material they will be relying upon, for as they would readily concede, Indian census data are not necessarily very reliable.

As at least one commentator at the conference noted, it is not an either-or affair. Qualitative and quantitative approaches may illustrate different realms. And the conclusions one reaches may not be terribly different, whether a more quantitative procedures are employed. Let me use the example of Hoxie's article on the Cheyenne River reservation before World War I, published in South Dakota History in 1979 ("From Prison to Homeland: The Cheyenne River Reservation Before World War I," reprinted in Iverson, The Plains Indians of the Twentieth Century, University of Oklahoma Press, 1985, pages 55-75). In this revisionist essay, Hoxie presents a picture of an Indian community that is strikingly different from the usual portrayal. It is a traditional narrative, but one that is original and persuasive in its reading of the evidence. Would the essential conclusions of this article be different if a more quantitative approach had been used? Would the author have decided that it was not a period of rapid cultural change, that the Cheyenne River Sioux were not capable of adaptation and innovation, that this era represented amidst difficulty and turmoil and ultimately other than triumphant transition? I don't think so.

In Calvin Martin's provocative anthology, there is a fine essay

by Henrietta Whiteman, a Northern Cheyenne. In "White Buffalo Woman," (Martin, Problem of History, pages 162-170) she speaks as the great grand child of a woman born in 1852, a critical time of overwhelmingly change. Whiteman says that Cheyenne history "is a continuum of sacred experiences rooted into the American landscape, with Bear Buffalo their most sacred and most powerful place...The Cheyennne sense of history," she continued, "is one of power, majesty, mystery, and awe. It is a sacred history, which has been well-preserved in the oral tradition." She doubts that Cheyenne history or American Indian history will ever "be incorporated into American history, because it is holistic, human, personal, and sacred. Whiteman refers to American history as white and secular. "In a brief five centuries," she wrote, "Anglo-American experiences have become a secular, scientific history without a soul or direction." (pages 169-170)

Thus Henrietta Whiteman, for one, does not want history to be a science. We can see why. For her and for many other Indians, that history lacks a soul and direction, is insensitive, does not appreciate a people's priorities, values, or traditions. As N. Scott Momaday reminds us in his "Personal Reflections," it often may mirror a lack of concern for language. When we rely upon "the legal diction of a special parlance, one that is far removed from our general experience of language...the words themselves seem to stand in the way of meaning." He would prefer a "plain style, a style that preserves, in its way, the power and beauty of language." (Ibid., page 161) And while Momaday specifically compared an executive order with the words of Satanta, his larger point was about language itself. Surely

history is a form of inquiry in which language has been and should be important. If we are to employ quantitative approaches, we need not abandon our commitment to clear, understandable prose--a commitment sorely lacking in much scientific writing. The social sciences may want to mimic the sciences. But in the zeal of imitation, they need not attempt to copy murky discourse.

Those of us who study American Indian history frequently advocate cultural pluralism. But if we are to embrace pluralism in our attitudes toward the world and its peoples, then we would do well to admit it in our own discipline and the approaches we may adopt. We need a few more scholars in the mode of my mentor at Wisconsin, Al Bogue. A pioneer in the area of social science history, he nonetheless let me go my own way.

Thus I see a place for mathematics in American Indian history. Yet there should be also a place for myth. There remains power and validity in poetry, just as there may be genuine significance in accumulated data. It depends, again, on the questions we ask, the answers we seek, and at times, the silences we understand--and our appreciation that things are not always as they seem.